Interpretation and Application

Epistemology of quantum theory and
the de Broglie-Bohm interpretation

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Kvanttimekaniikan filosofia ja etenkin keskustelu kvanttimekaniikan tulkinnoista ovat säilyttäneet pääpiirteiset erimielisyytensä kvanttiteorian alkuajoista asti nykypäivään. Tässä tutkielmassa täsmennetään kiistan ydinkysymyksiä ja etsitään mahdollisuuksia vastata niihin analysoimalla tulkinnan käsitettä.

Kysymysten kehystämiseksi esitellään kaksi perinteistä tulkintaa kvanttiteoriasta, Kööpenhaminan tulkinta ja de Broglien-Bohmin tulkinta. Metodologisena valintana tulkintoja käsitellään rajatusti tulkintoina kvanttiteoriasta, ei erillisinä teorioina. Kööpenhaminan tulkintaa käsitellään kvanttiteorian perusmateriaalin sekä tulkinnan 1930-luvun oleellisimpien kirjoittajien, Werner Heisenbergin ja Niels Bohrin teosten avulla. Kööpenhaminan tulkinnan argumentoidaan olevan kvanttiteorian tulkintaa vain triviaalissa mielessä. Sen teoreettinen sisältö eroaa häviävän vähän itse kvanttiteoriasta ja sen rajat ylittävän spekulaation hylkäämisestä. De Broglien-Bohmin tulkinta esitellään Louis de Broglien pilottiaaltoteorian ja myöhemmän David Bohmin ontologisen tulkinnan kautta. Tulkinta formuloi teorian matemaattisen ytimen vaihtoehtoisella tavalla – käyttämällä aaltofunktion polaarista muotoa niin sanotun kvantti-Hamilton-Jacobin johtamiseksi Schrödingerin yhtälöstä, mikä mahdollistaa kvanttiteorian standardimuotoilusta poikkeavien termien eristämisen. Tulkinta säilyttää kvanttiteorian empiiriset ennusteet tuottavan rakenteen.

Kahden eri tulkinnan kannattajien välisen kiistan täsmennetään syntyvän epäselvyydestä ja erimielisyydestä tulkinnan arvioinnin standardeista, kuten yksinkertaisuudesta, kuvausvoimasta tai periaatteellisista mahdollisuuksista muodostaa uusia tutkimushypoteeseja. Kiistan ratkaisemiseksi tutkielmassa edetään purkamaan itse tulkinnan käsite. Sen käyttöä tutkitaan fysiikan mallikeskustelun, semanttisen logiikan ja matemaattisen malliteorian yhteyksissä. Tutkielmassa päädytään esittämään ehdotus käsitteen ”tulkinta” reunaehdoille: tulkinta tarkoittaa teorian soveltamista haluttuun yksittäistilanteeseen. Tulkinta kvanttiteoriasta voi siis olla oikea tai väärä, minkä määrittää soveltamisen onnistuminen. Reunaehtoihin sisällytetään yksittäisten mallikomponenttien arviointi, mikä mahdollistaa vaihtoehtoisten matemaattisten muotoilujen sisältämien termien arvioinnin kvanttiteorian soveltamisen yhteydessä.

Koska tulkintoja käsitellään tutkielmassa yksinomaan kvanttiteorian tulkintoina, päädytään johtopäätökseen, että tulkintojen on jaettava kvanttiteorian rakenteelliset rajoitteet. Nämä rajoitteet ymmärretään tätä nykyä hyvin viimeistään von Neumannin ja Kochenin-Speckerin teoreemien myötä. Lisäksi tutkielmassa asetetut reunaehdot kvanttiteorian tulkinnalle ovat hyvin tiukat. Tämän takia vaihtoehtoja tavanomaiselle ymmärrykselle siitä, mitä kvanttiteoria sanoo todellisuudesta, on syytä tarkastella laajemmassa viitekehyksessä.

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CONTENTS

I. INTRODUCTION

Interpretations of quantum mechanics have become a major point of interest not only in the field of physics, but in other disciplines and popular culture as well. For example, even philosophy of mind and cognitive science have drawn inspiration from the speculations regarding the causal properties of quantum systems.

However, there are clear differences in opinion regarding what an interpretation should and should not do, which leads to vastly different world views. Different weights are given to different attributes of the theory: for example, it can be deemed that indeterminism in physics is unacceptable, leading to theories that preserve it. Alternatively, the necessity of local dynamics might be emphasized, motivating a compatible interpretation, and so on.

It indeed seems that this discussion is kind of a Wild West in the philosophy of science. There are contradicting preferences with often little mutual agreement on the standards against which to judge them. In this thesis, I will examine two well-known interpretations of quantum theory, namely the Copenhagen interpretation and the de Broglie Bohm interpretation. The choice is mainly motivated by the distinctions in epistemology and ontology between them. The Copenhagen interpretation can be characterized as restricting statements to what can be known by the means of observation. The de Broglie-Bohm interpretation is selected as a contrast to the Copenhagen for its hypotheses of ontology.

However, the examination here is not a one-to-one comparison between all possible properties belonging to these two lines of thought. The contrast is utilized in order to find clarity to a more general question concerning the philosophy of quantum physics: are there robust scientific *or* philosophical standards by which disputes between contradicting interpretations could be reliably solved? The de Broglie-Bohm interpretation is selected as a tangible example of a *realist* interpretation, that is, an interpretation with a commitment for the search of ontology. However, there are other such interpretations as well.

To gain footing in this project, the whole concept of interpretation is taken apart and put back together. As will be shown, there is an apparent disregard to any substantive standards pertaining to what constitutes an interpretation of quantum theory: instead, it is commonly agreed that an interpretation is any theoretical framework that accounts for what happens in sub-atomic systems. Interpretations are described in the Stanford

Encyclopedia of Philosophy article *"Quantum Mechanics"* (Ismael 2000) in the following way:

> Minimally interpreted, the theory describes a set of facts about the way the microscopic world impinges on the macroscopic one, how it affects our measuring instruments, described in everyday language or the language of classical mechanics. Disagreement centers on the question of what a microscopic world, which affects our apparatuses in the prescribed manner, is, or even could be, like intrinsically; or how those apparatuses could themselves be built out of microscopic parts of the sort the theory describes.
>
> That is what an interpretation of the theory would provide: a proper account of what the world is like according to quantum mechanics, intrinsically and from the bottom up.

This is a starting point for this examination, but as a qualifying ruleset, nowhere near sufficient. Consequently, a robust proposition for rules of *what counts as an interpretation of quantum theory* will be attempted in this thesis.

In the second chapter, the history of quantum physics will be summarized. I'll start from the early observations of light leading up to the models of light as electromagnetic fields. Then, the concept of photons is introduced as the first example of wave-particle-duality, and the idea is extended to electrons through the de Broglie matter waves. Leading up to the modern theory, the Schrödinger equation is presented along with the alternative formulation provided by matrix mechanics.

The rest of the second chapter is dedicated to the Copenhagen interpretation, the uncertainty principle and methodological discussions of the 1920's and 30's. It will be seen that through this discussion, the current understanding of the epistemic limitations of quantum theory was solidified.

The third chapter is solely a presentation of the de Broglie-Bohm interpretation, using as the main source David Bohm and Basil Hiley's 1993 book *"The Undivided Universe",* which encompasses the most elaborate version of the de Broglie-Bohm interpretation by its original author.[1] Later developments, especially in the field of Bohmian mechanics, will not be studied as they fall outside the scope of this thesis.

The second half of the third chapter discusses the conflict between the Copenhagen and the de Broglie-Bohm interpretations, as well as general criticisms received by the latter. In an important role are so-called "no-go theorems" which aim to prove the

[1] An exhaustive treatment is also provided by Holland (1993).

impossibility of hidden variables in quantum mechanics. Their role in limiting the scope of what is possible in the context of quantum mechanics is clarified: they inevitably rule out improvements in accuracy in the domain of quantum theory – but do not claim to exclude the entirety of so-called “hidden variable interpretations”[2]. The two theorems addressed – the von Neumann proof and the Kochen-Specker theorem – are also seen to operate on slightly different domains. While the von Neumann proof has to do with the basic characteristics of *any* theory pertaining to quantum mechanics, the Kochen-Specker theorem addresses the absolute limits of quantum theory itself. It will also be concluded that the de Broglie-Bohm is not a separate theory (from quantum theory) because of the equivalence of its predictions.

The fourth chapter is devoted to finding out what an interpretation of quantum theory is. I start by examining some general texts regarding models in physics and attempt to identify the kinds of models that are relevant to subatomic phenomena. After getting some pointers, I will move forward to examine the notion of interpretation. At this point, we already have some sort of an understanding of an interpretation – which the relation between a model and the subject it represents. This will be scrutinized further by examining basic semantics and then moving to model theory and some attempts at a formal system of empirical models. Relevant ques are taken here without commitment to any particular framework, for example that of mathematical model theory.

These ideas are then briefly measured against further discussion on models in physics, after which I present my final formulation of how interpretation, and interpreting, could be generally understood in the domain of quantum theory. Most importantly, it is proposed that an interpretation is the act of understanding the theory’s reference to phenomena. Having given the set of statements regarding what an interpretation is, the Copenhagen and de Broglie-Bohm interpretations are both shown to be an ill fit for the concept. The Copenhagen interpretation is essentially nothing but a set of statements that are immediate consequences of the empirical limitations of quantum theory. The de Broglie-Bohm interpretation, respectively, abides by the rules of interpretation (as suggested in this thesis) only when crucial ontological statements regarding its components are omitted.

Central to everything that is discussed is the axiom that there is only one quantum theory and differing interpretations (in the accustomed sense) of quantum theory,

[2] The notion of “hidden variables” is intentionally left in quotations because of its loose definition – more on this in III.III.

whatever their actual nature may be. Thus, the no-go theorems pertaining to quantum theory itself are given a strong emphasis – if these theorems were to be worked around, in any shape or form, there is strong reason to argue that this would mean a completely new theory of sub-atomic phenomena.

The goal of this thesis is ambitious in the sense that I seek to give a proper definition of interpretation, and interpretations, in the domain of quantum theory. However, nothing far-reaching is attempted here – the main commitment is to *stick to what we know for a fact* at all times. This does not exclude philosophy, nor does it say anything general about philosophy of physics – but, hopefully, a better picture of *what can be said of physics itself* will arise.

II. A BRIEF HISTORY OF QUANTUM THEORY

To understand the problems discussed in this thesis, it is necessary to go over the fundamentals of quantum mechanics. I will first briefly look at the core experiments performed in the late 19th and early 20th centuries that laid the groundwork for modern quantum theory. After this, I will describe the basic wave equations and concepts that hold in current quantum mechanics.

Some special attention is given to matrix mechanics and operator formalisms. The motivation for this is that Schrödinger's wave mechanics follows more intuitively from the preceding experiments and models, whereas matrix mechanics treat observables in a different picture[3]. This picture is, however, important to cash out, as the mathematical treatment is illuminating in the context of interpretations of quantum physics.

Finally, I will address the wave function as it is central to most quantum mechanics and show how the motivation for interpretations arises from ontological issues associated with it. The origin of the received view of quantum mechanics, the Copenhagen interpretation, is briefly discussed due to its close relation to the development of fundamental models of modern quantum theory.

[3] The notion of 'picture', as will be expanded later, is a semi-technical term relating to the mathematical framework being utilized.

II.I. EARLY EMPIRICAL FINDINGS

Following Richard Feynman (Greene 1999), it is fruitful to approach quantum mechanics as we know it today by examining experiments on light. The wave nature of light was first discovered by Thomas Young (1802) by simple experiments involving paper and sunlight. Contradicting the then-prevalent corpuscular theory of light, in which light was modelled as travelling in straight rays, light was found to exhibit wave-like attributes when directed through small enough slits. Observing light on the other side of one slit, the pattern formed by it was found not to be of the shape of the slit, but rather a diffraction pattern with minimums and maximums of intensity. Similarly, an experiment with two or more slits produced more elaborate diffraction patterns, which were fully in line with a model of interference between wave fronts.

Young's paper led physicists to adopt the wave theory of light, which simply stated that light has a *wave-nature*. This was to say that patterns found in other wave motions, such as diffraction of waves in fluids, were found to be similar with the behaviour of light. James Clerk Maxwell (1865) proceeded to show that light is a propagating wave exhibiting both electric and magnetic qualities, or in other words, electromagnetic radiation. This, among other things, lead to an understanding that light carries energy with it.

The question of electromagnetic radiation was linked to temperature when, later in the 19th century, physicists endeavoured to explain the relation of between the intensity and wavelength of the radiance that physical bodies emit. This is what later came to be called the *black body problem*. Kirchhoff (1860) explained that a body of a given shape and size in a thermal equilibrium[4] has a universal ratio between the coefficient of absorbed radiance and emitted radiance, which only depends on wavelength and temperature. The coefficient is given by the ratio between reflected and absorbed light, where for a perfectly black body all radiation will be absorbed (i.e. a perfectly black body does not reflect any light). This yields *Kirchhoff's law* which states that for a given wavelength, the amount of radiation absorbed equals the amount emitted. This can be stated mathematically as $\alpha\lambda = \epsilon\lambda$.

However, the exact relation between wavelength and intensity was not known. Rayleigh and Jeans (1905) constructed a model which stated that as the wavelength of emitted

[4] Thermal equilibrium is a state where there is no flow of states in a system from less entropic to greater. In other words, the examined system has an equal temperature in all its regions.

radiance decreases, its intensity increases as an exponential function. In terms of wavelength, it was written as (Rybicki & Lightman 1979, 20—27):

$$B_\lambda(T) = \frac{2ck_BT}{\lambda^4},$$

where $B_\lambda(T)$ denotes the power emitted per unit emitting area, per unit wavelength, per steradian, c the speed of light, k_B the Boltzmann constant, T the temperature and the λ wavelength. The important feature of this equation is that the power emitted is proportional to λ^{-4}, which is determined by empirical testing.

The problem of the Rayleigh-Jeans model was that empirical findings were obviously against it. According to the model, when moving towards shorter wavelengths, and thus towards higher frequencies, the spectral radiance of the body starts to quickly approach infinity. In reality, this is not and cannot be the case, because the energy radiated by a body must be finite. Moreover, the amount of radiated energy would violate the principle of conservation of energy. A model with a solution to this problem (which is sometimes coined “the ultraviolet catastrophe”) was, however, given earlier by Planck. In his (1900b) paper he proposed as a solution his equation:

$$B_\lambda(\lambda, T) = \frac{2hc^2}{\lambda^5}\frac{1}{e^{\frac{hc}{\lambda k_BT}} - 1},$$

where *h* is the Planck constant. The relation is still inversely reactive to wavelength. However, here the inclusion of the Planck constant ($6.626 * 10^{-34}\ J\ s$) results in the equation for radiance converging towards zero after around $\lambda = 0.5\ \mu m$, as confirmed by experimental results.

The significance of *h*, the Planck constant, should be emphasized. Although originally it was seen as a mere mathematical tool needed to derive proper predictions by means of quantifying over discrete values, the physical meaning of the constant became more robust in Einstein’s theory of the *photoelectric effect*. The photoelectric effect refers to the emission of electrons by a conductive material when hit by light. According to classical wave theory, the kinetic energy of electrons emitted should be proportional to the energy carried by the light that is directed at the material, that is, its intensity.

Empirical tests showed this to not be the case. Instead, two key observations of the phenomenon needed clarification. First, it was observed that no electrons are emitted below a certain frequency of light. This is called the *threshold frequency*. Secondly, when going above the threshold frequency, it was shown that the kinetic energy of emitted electrons was independent of the intensity of light, but dependent on its frequency by positive correlation. Einstein (1905) developed a mathematical model encompassing these observations:

$$K_m = hf - \varphi$$

$$\varphi = hf_0$$

$$K_m = h(f - f_0)$$

Here, K_m corresponds to the maximum kinetic energy carried by the electron, and φ is a work function denoting the minimum work required to move the electron. Given the threshold frequency, the work function can be written as the product of the Planck constant[5] and the minimum frequency for the emission as found by experimentation. The quantization used in the black body radiation model quantizes the kinetic energy states of electrons (the Planck constant is utilized in both). To note is that the energy itself *is* still a continuum above the threshold frequency – it's just zero below it. This led to early notions of discrete values for some properties of light, contrasting to the continua in classical wave theories. The explanation, proposed by Einstein, was that light is transferred as "packets of energy" that are indivisible[6]. This idea is considered to be fundamental to the early development of quantum mechanics.

I will now turn to the development of atomic theory. Building from early phenomenological models towards a more fundamental theory, Bohr and Rutherford (1913) formed the first functional theoretical model of a hydrogen atom. Following the discovery of the atomic nucleus in Geiger-Marsden experiments of 1909, combined with earlier understanding of electrons, Rutherford started to sketch an early model of the hydrogen atom (1911). His model consisted of a solid nucleus and a cloud of electrons around it, in a motion resembling planetary orbits. The model was structurally somewhat close to current understanding of the hydrogen atom, but it was plagued by problems due to the classical predictions it entailed. Because the electromagnetic theory of radiation tells that electrons in motion emit radiation, the law of conservation of energy dictates that the total energy of the electron must diminish accordingly. The energy emitted by radiation is due to the electron's kinetic energy. Therefore, according

[5] The relation *E=hf* was already introduced by Planck in 1901.
[6] The same idea of quantization of light is a physical explanation of the black body phenomenon as well, as formulated by Planck.

to the model, the electron should “fall” to the nucleus – leading to the instability of all atoms.

Bohr’s solution to this problem was to quantize the orbits of the electrons. This solution combined the earlier approach to the electron’s energy levels by classical modelling and a quantum boundary condition:

$$v = \sqrt{\frac{Zk_e e^2}{m_e r}}$$

$$E = -\frac{1}{2} m_e v^2$$

$$m_e v r = n\hbar, \qquad n \in \mathbb{N}$$

Here, m_e denotes the mass of the electron, e its charge, Z the charge number of the nucleus (i.e. the atomic number) and k_e is Coulomb’s electrostatic constant, E is the total energy of the electron and v its velocity and n is the electron's orbital quantum number. The first two of the equations above are classical models for velocity and energy of the electron in orbit around the nucleus, whereas the third one describes a quantum boundary condition.

In a rough analogy with planetary orbits, the velocity of the electron depends on the product of its charge with that of the nucleus, as well as its distance from it. The total energy takes the classical form of a kinetic energy equation. However, the values of the electron’s angular momentum ($m_e v r$) are strictly discrete, so that they can only be multiples of the reduced Planck constant[7]. The physical meaning of this is that there must be discrete solutions for the radius r, and thus for the corresponding energy states E. No states outside of those that are dictated by $n\hbar$ are allowed. To satisfy the conservation of energy, the electron at a stable orbit does not radiate energy. The lowest state, $\hbar$, corresponds to the lowest possible orbit, thus making atoms in the model stable. Energy is absorbed or emitted as radiation only by switching orbits, and the corresponding energies of these transitions can be determined by simple calculations of energy differences of orbits.

[7] $\hbar = \frac{h}{2\pi}$. This defines the quantum constant for angular momentum.

II.II. THE MODERN THEORY

Overarching early quantum theories of light and matter is a duality of wave- and particle-like properties. The Planck constant found in the cases of black body radiation, photoelectric effect and the hydrogen atom model suggests indivisible units of light which do not conform to classical wave-like models. The idea of such undivided units as a physical property was gradually accepted towards the early twenties and demonstrated further in 1923 by Compton scattering. In this experiment, electrons are bombarded with light of higher energy than in the photoelectric effect (Compton, for instance, used an X-ray beam), so that the light is not fully absorbed but rather scattered inelastically, causing the electrons to recoil.

A classical wave model of light necessitates that without any external or emergent forces present, the scattered light must have the same wavelength as the incident light. In Compton's experiment, however, it was found that the scattered light demonstrates a shift to a higher wavelength. This can be explained by the earlier prediction from the photoelectric effect, $K_m = hf$, but applying the kinetic energy relation to the scattered light in addition to the recoiling electron. If there is a physical particle in the incident light colliding with the electron, the wavelength shift can be explained by the loss of energy of scattered particles. This is turn would lead to a lower frequency of the scattered light. Finally, these particles of light were coined ‘photons’ by Gilbert Lewis in 1926.

Even though the physical existence of both wave- and particle-like behaviour was evident, the exact relation between the two was not clear. Especially perplexing was that photons exhibited wave-like behaviour; this could not be reconciled with classical electromagnetic theory. This question can be framed as the first fundamental question of quantum mechanics and will be returned to later on. However, another question was posed about the possibility of electrons exhibiting wave-like behaviour. This presented the possibility of finding a unified underlying empirical theory.

The first physicist to hypothesize about such wave-like properties of electrons was Louis de Broglie in 1924. His idea was that the relations between photons and waves in light are universal to all particles. His fundamental equation describing the properties of so-called matter waves[8] linked the momentum of an electron to a wavelength:

$$\lambda = \frac{h}{p} = \frac{h}{mv}$$

This is to be understood as a generalization of the relation already observed in the photoelectric effect, where the momentum of the electron (necessitating a corresponding momentum of the photon) is dependent on the frequency of the light. This equation proved to yield correct experimental predictions.[9] The natural question then was: if electrons exhibit wave-like behaviour, is there a corresponding wave equation to be found?

II.II.I. THE SCHRÖDINGER EQUATION

Erwin Schrödinger proposed a quantum wave equation in 1926, partly inspired by Hamilton's principal function[10]. The model offered was a linear partial differential equation, which could predict the total energy states of the system by operating on a *function describing a wave*. Additionally, the equation had to encompass development over time. Otherwise, it could not fully describe the transitions of electron orbits. This equation could not be mathematically derived from other fundamental equations of physics, but was construed through logical arguments, de Broglie relations and close study of preceding experimental results particularly regarding the hydrogen atom. Schrödinger's solution for a non-relativistic particle was finally presented as:

$$i\hbar \frac{\partial}{\partial t} \Psi(\vec{\mathrm{r}}, t) = \left(-\frac{\hbar^2}{2m} \nabla^2 + V(\vec{\mathrm{r}}) \right) \Psi(\vec{\mathrm{r}}, t),$$

where *i* is the imaginary unit and $V(\vec{r})$ the potential of the system. $\Psi(\vec{\mathrm{r}}, t)$ represents the wave function with $\vec{\mathrm{r}}$ being the wave displacement. In his 1926 paper Schrödinger proceeded to show that this equation was able to reproduce the spectral series of the hydrogen atom. This was accomplished by modelling the electron as the wave function and the attraction caused by interaction with the proton by the potential term *V*.

[8] For de Broglie, the fundamental relation between waves and particles did not mean they refer to the same objects: the matter wave concept was understood by him to model two different entities and served as the basis for the pilot wave theory.

[9] See Thomson (1927).

[10] I.e. the Hamilton-Jacobi equations. These unified Newtonian notions of particle mechanics and optics and were able to represent particle motion by wave equations.

Although lacking a relativistic correction, the fact that it correctly predicted the behaviour of particles as a wave equation made it the most utilized equation of all quantum physics.

Despite the equation’s predictive accuracy, the nature of the wave function itself was early on recognized as a question regarding the Schrödinger equation. Even though its form can be derived from the differential equation, it does not directly correspond to observables: all predictions regarding observations are results of operating on it; such as probability densities and expectation values. The fact that the Schrödinger equation’s form resembles that of a classical wave equation led to early ontological questions regarding the nature and behaviour of objects in quantum mechanical systems. But before these can be discussed further, a summary of another approach of a unified formalism in quantum mechanics is still needed.

II.II.II. MATRIX MECHANICS

Slightly before the time of formulation of wave mechanics, another form of quantum mechanics encompassing time-dependency was moulded by Heisenberg, Born and Jordan (1926) with preceding contributions from Heisenberg (1925) as well as Born and Jordan (1925). Their version of the quantum theory was called ‘matrix mechanics’ after its treatment of physical properties of particles by matrices. In order to avoid the difficulties of summarizing the content of Heisenberg’s original 1925 paper, I will outline instead the modern core formulations of matrix mechanics and its operators along with their physical references. Ques are taken from Ludyk (2018).

The core idea of matrix mechanics is to work within an operator formalism which corresponds only to known observables and their probability distributions (along with expectation values). This is a break away from speculative models such as the hydrogen atom model[11]: in matrix mechanics, no underlying physical structures are assumed which cannot be directly observed. What this means in more concrete terms is that the

[11] The hydrogen atom model is heuristic but speculative – it is not observed for a single electron that the nucleus is orbited by it, or an electron cloud for that matter. Instead, these are tangible pictures that are given for statistical rules for predicting energy states from localizations of wave packets, etc. In terms empirical results, the physical idea of the orbital nucleus-electron system is not warranted. However, the electron orbit *can* be constructed with an ensemble of a large number of electron detections (e.g. Stodolna et al. 2013).

mathematical picture does not include any terms referring to wave or particle motion, for instance. Instead, statistical patterns are inferred from operator evolutions alone.

The reason the outlines of matrix mechanics are presented here is the fact that this framework neither supposes nor suggests anything of any kinds of wave or particle natures of quantum systems, atomic models, and so forth. Instead, only observed states are predicted in an operator formalism. This is to showcase the fact that to correctly cash out all the empirical content of subatomic phenomena, no commitments to anything else than observation is required. Moreover, some properties of the phenomena as described by quantum theory can be understood to be *inevitable* results of the mathematical framework they are included in. These themes will be discussed in greater detail in III.III.

The fundamental equations of matrix mechanics start with the explication of the so-called uncertainty principle, as observed by experimentation. In terms of physical phenomena, the uncertainty principle states that two (non-commuting) observables[12], such as position and momentum, cannot be simultaneously observed with arbitrary accuracy, but that increasing accuracy for one decreases accuracy for the other.[13] In matrix mechanics, this is stated as a commutation relation:

$$[\hat{x}, \hat{p}] = \hat{x}\hat{p} - \hat{p}\hat{x} = i\hbar$$

This residue is caused by the matrix form of the operators $\hat{x}$ and $\hat{p}$. Non-commutative pairs of operators simply entail that their order of operation cannot be chosen arbitrarily. The commutator above is presented above with mere operators, but in real solutions they operate on a function. For example:

$$[\hat{x}, \hat{p}]|\psi\rangle = (\hat{x}\hat{p} - \hat{p}\hat{x})|\psi\rangle = \left(\hat{x} - x_0\hat{I}\right)\hat{p}|\psi\rangle = \ i\hbar|\psi\rangle,$$

where $|\psi\rangle$ denotes the vertical eigenstate vector of position (i.e. the eigenvector, or the state vector) with the constant eigenvalue x_0, which can be defined in three-dimensional space by the relation

[12] By ‘observables’ I mean quantities that directly correspond to phenomena that can be observed. This term is applied while recognizing the disagreements associated with it. For an exposé of the subject, see Bell (1987, 52—62).

[13] It will shortly be explicated that this loss of accuracy is not due to shortcomings of measurement equipment but an inseparable attribute of quantum mechanics itself. See II.II.III.II.

$$\hat{x}|\psi\rangle = x_0|\psi\rangle = (x_x \quad x_y \quad x_z)\begin{pmatrix}\psi_x \\ \psi_y \\ \psi_z\end{pmatrix},$$

where we find x_0 as the eigenvalue if it yields the product of $\hat{x}$ and $|\psi\rangle$ by scalar multiplication. This expression is equivalent to $(\hat{x} - x_0\hat{I})\,|\psi\rangle = 0$, which is of the form we find from the canonical commutation relation. Here, $\hat{I}$ is the identity operator in three dimensions, of the matrix form:

$$\begin{bmatrix}1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1\end{bmatrix}$$

From these equations we can see that if $|\psi\rangle$ were the eigenstate of momentum as well, the result would be similarly zero. Now that the canonical commutation relation is understood, the job becomes to show how to derive the energy states found from the orbital transitions of electrons. This overview will be done in some detail, because it is an efficient demonstration of matrix mechanics. I will start by assigning an operator *A* for an observable *a*, as previously done for position and momentum, and defining it as the expectation operator by the integral

$$\int \psi_m^* A_{mn} \psi_n \, dx.$$

which, by integrating over *m*, gives the expectation value for the eigenstate ψ_n to return the eigenvalue of energy E_n.[14] In this formalism, time evolution is in respect to the operators and not the state vectors they operate on. The operator *A* here is defined as the combined matrix of operators $\hat{x}$ and $\hat{p}$. The core idea is that the time differential of *A* relates to energy states by the equation:

$$\frac{dA_{mn}}{dt} = i(E_m - E_n)A_{mn} = iE_mA_{mn} - iE_nA_{mn}.$$

Which, introducing the Hamiltonian operator *H* for total energy, can be stated as:

$$\frac{dA}{dt} = i(HA - AH) = iHA - iAH.$$

From the above equations it can be seen that *A*, as a function of time, remains constant to the value given by its initial state. Recognizing this, the complete formulation of Heisenberg's equation of motion can be given as a commutation relation:

[14] Notations n and m refer to different eigenvalues of energy.

$$\frac{dA}{dt} = i\hbar[H, A] + \frac{\partial A}{\partial t},$$

where the partial derivative term is for operators that have direct time dependence in addition to the time evolution of the operators in the commutator.

II.II.III. THE COPENHAGEN INTERPRETATION AND RECEIVED VIEW OF QUANTUM THEORY

As previously presented, the two different pictures of quantum theory presented here (wave and matrix mechanics) involve different mathematical frameworks from which the same experimental results can be derived[15]. Due to the uncertainty principle, wave-particle dualism and quantization of measured states, the early years of quantum theory involved fundamental and philosophical discussion regarding the correct ontological attitudes taken towards the new phenomena and theories about them. The topic of this discussion came to be called the "interpretations" of quantum mechanics. Early on, the received view formed around the views of Niels Bohr and Werner Heisenberg (as well as Max Born) in the latter part of the 1920s. This framework would come to be called the "Copenhagen interpretation"[16]. However, after a significant amount of literature written on the subject in the course of the 20th century, the term cannot be treated as well-defined: a multitude of often-conflicting stances in the context of interpretations of quantum mechanics have been presented that are categorized under "Copenhagen" (Faye 2019, 8).

In order to preserve accuracy, the fundamental ideas shared by both Bohr and Heisenberg shall be treated as the "Copenhagen interpretation" in this thesis. The textbook "*The Physical Principles of the Quantum Theory*" (Heisenberg, 1930) will be used as the primary source. It's also important to note that areas where Bohr and Heisenberg disagreed will not be included here.

One of Heisenberg's goals in the book was to "contribute somewhat to the diffusion of that *Kopenhagener Geist der Quantentheorie*" (Heisenberg 1930, preface). Thus, even though the aim here is to provide a systematic and tangible view of the stances in the

[15] It needs to be stated that both wave- and matrix mechanics have *equivalent* empirical content. Moreover, the systems are *mutually isomorphic* (von Neumann 1932, 17—27).
[16] The term "Copenhagen interpretation" was later coined by Heisenberg while criticizing alternative interpretations. (Kragh 1999, 210.)

Copenhagen interpretation, the origin of the term refers more to a general spirit than a defined ruleset.

For Heisenberg, one of the fundamental notions regarding the uncertainty principle is that the wave-particle-duality, as seen in the classic experiments, appears as physical because of the limitations of language. Because language has evolved to describe and interact with everyday experiences, it is in pains attempting a description of more intricate atomic processes. This has to do, as well, with the notion of mental images: as Heisenberg argues, the language we use to describe phenomena constructs a mental picture of the physical system in question. Due to the limitations of language, these pictures have their limitations as well – resulting in the limited and contextual use of both wave- and particle pictures, depending on their applicability. Fortunately, mathematics is not as hindered by these limitations; and thus, the quantum theory can be presented as complete in its mathematical form. Heisenberg writes (Heisenberg 1930, 10):

> The solution of the difficulty is that the two mental pictures which experiments lead us to form – the one of particles, and the other of waves – are both incomplete and have only the validity of analogies which are accurate only in limiting cases. It is a trite saying that “analogies cannot be pushed too far”, yet they may be justifiably used to describe things for which our language has no words.

Heisenberg, in discussing quantum statistics, presents a good heuristic map of strengths and weaknesses of two opposing stances regarding quantum phenomena (Heisenberg 1930, 65). One of the fundamental differences of quantum statistics, as opposed to classical statistical physics, is that the fundamental notion of causality is lost, as long as referring to spacetime phenomena is prioritized. This is because of the uncertainty principle (which will be addressed in greater detail briefly): as position and momentum are complementary, no measurement can be made to confirm a definite causal history for either (a definite causal history would require a simultaneous measurement of both quantities). It is, however, possible to describe causal relationships by means of the mathematics of quantum mechanics, but in this case reference to physical spacetime has to be abandoned.

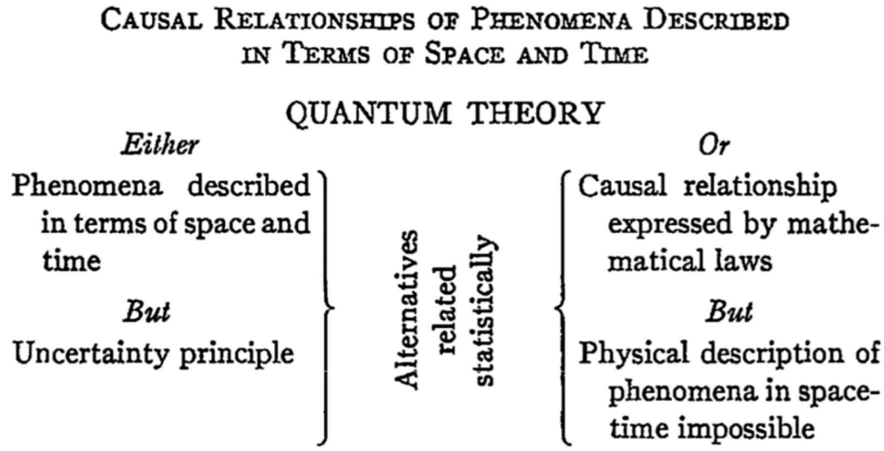

Figure 1: Heisenberg's illustration of statistical alternatives (Heisenberg 1930, 65).

Further, in Heisenberg's view, it is meaningless to discuss probabilities in quantum statistics without reference to the experiments producing the probability distributions. This is to say that one cannot, in principle, separate the measured system from the measuring device. For quantum statistics, this maxim entails that any values given by a mathematical apparatus, without being directly related to experimental phenomena, have no physical reference. Heisenberg writes (Heisenberg 1930, 57):

> The statistical relation by means of probability coefficients is determined by the disturbance of the system produced by the measuring apparatus. Unless this disturbance is produced, there is no significance to be given the terms "value" or "probable" value of a variable in a given direction of unitary space which is not parallel to a principal axis of the corresponding tensor. Thus one becomes entangled in contradictions if one speaks of the probable position of the electron without considering the experiment used to determine it.

While a restriction on the significances of statistical variables, this passage is representative of the general idea of the "Copenhagen spirit" – unobserved quantities or qualities, generally, cannot have a physical significance.

II.II.III.I. THE UNCERTAINTY PRINCIPLE

As discussed under the section on matrix mechanics, central to the limitations posed by the particle and wave pictures of quantum mechanics is the uncertainty principle. For the proponents of the *Copenhagen spirit*, the principle is to be treated as fundamental, and not as a contingent limitation of experimental apparatuses.[17] Although already laid

[17] There is a connection to Bohm's thinking here – more on this in III.III.

out in the context of canonical commutation relations, a more physical exposition of the principle will be presented here.

The uncertainty principle states that the position and momentum of a particle cannot be simultaneously known with arbitrary accuracy. Here “position” is a localization of a wave packet. If the position of a particle is known at a certain accuracy Δx, then Δx corresponds to the extension of the wave packet. A wave packet, in contrast to a wave, refers to a wavelike disturbance, which has a non-zero amplitude only in a bounded region, regardless of its physical nature. The wave packet moves through space with velocity v, but this cannot be accurately defined because of the diffusion of the wave packet. (Heisenberg 1930, 13.)

Because momentum in the x-axis is $p_x = mv_x$, the uncertainty Δv_x in velocity causes the similar uncertainty Δp_x for momentum. As the smallest possible extension of the wave packet corresponds to its wavelength λ, and we already have established the relation[18]:

$$\lambda = \frac{h}{p}$$

we are able to derive the original uncertainty relation[19]:

$$\Delta x \Delta p_x \geq h$$

It is useful to note here that a more general expression of the uncertainty relation in physics is written in terms of standard deviations of position and momentum (Kennard, 1927):

$$\sigma x \sigma p \geq \frac{\hbar}{2},$$

which is equivalently true for observations of the electron. Accordingly, the uncertainty relation can also be stated, and derived, without reference to a wave picture. (Heisenberg 1930, 15–20.)

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[18] See page 10.

[19] The difference between the terms "uncertainty relation" and "uncertainty principle" is that the former refers to a mathematical relation, and the latter to a physical principle that is derived from the former.

II.II.III.II. LIMITATIONS OF THE WAVE AND PARTICLE PICTURES

The uncertainty principle is the baseline for sketching the borders where the treatment of electrons as localized point masses can be applied. From the relations presented above, it is already clear that $\Delta x \Delta p$ has a limit of accuracy – a limit on how *localized* our knowledge of the electron's state can be at a given time t. It is important to notice that the uncertainty principle says nothing about the *history* of the electron. Accordingly, singular measurements of either position or momentum can be carried out with arbitrary accuracy. Heisenberg clarifies:

> Thus suppose that the velocity of a free electron is precisely known, while the position is completely unknown. Then the principle states that every subsequent observation of the position will alter the momentum by an unknown and undeterminable amount such that after carrying out the experiment our knowledge of the electronic motion is restricted by the uncertainty relation. (Heisenberg 1930, 20)

In this situation, the history of the electron may be calculated, in order to get a theoretical model of both position and momentum of all points of time preceding the measurement. But as every measurement affects the system, this knowledge cannot be used to predict the electron's location at points $t + \Delta t$. As the history of the electron cannot be used to predict its future, it remains speculative in character:[20]

> It is a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or not. (Heisenberg 1930, 21.)

Heisenberg presents several experimental examples to illustrate the unavoidability of the restrictions the uncertainty relation imposes on the particle picture. I will discuss two of these restrictions. To begin with, we have the simple example of position determination by a microscope. The resolution of any microscope is determined by

$$\Delta x = \frac{\lambda}{\sin \epsilon},$$

where the term ϵ denotes the angular opening of the light cone, imposed by the microscope from a given distance. For the microscope to detect the electron, at least one photon must hit the electron, and travel through the microscope to the observer. Because the direction of the scattered photon is undetermined, the recoil received by the electron

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[20] Claims that some experiments bring forth this history, especially in the context of so-called *weak measurements*, have been made. However, these kinds of claims are inaccurate if a violation of the uncertainty principle is implied. See e.g. Kastner, 2017.

is accordingly undetermined. Thus, at the immediate moment when the measurement is made, the uncertainty relation (for the electron's momentum in the x-axis) $\Delta x \Delta p_x \sim h$ kicks in.[21]

The classic single-slit experiment is an attempt to avert wave properties in experimentation. In the experiment, photons are fired in the x-axis through a slit of length d in the y-axis. Assuming that the situation is two-dimensional, and the electrons have no momentum in the y-direction, the uncertainty of the electrons' positions in the y-axis after passing through the slit is known to be $\Delta y = d$, while the equation $p_y = 0$ should still hold for momentum. But, as the electrons behave as de Broglie matter waves, they are diffracted when passing through the slit. This creates momentum in the y-direction, which is uncertain by the amount

$$\Delta p_y = \frac{h}{\lambda} \sin \alpha,$$

where $\sin \alpha \sim \lambda / d$ is the angle of divergence of the beam having passed the slit. From this we get the uncertainty relation once more. (Heisenberg 1930, 21–24)

As with the particle picture, quantum mechanics imposes non-classical limitations to the use of the wave picture accordingly. Heisenberg notes that it is important to understand that notions of wave amplitude refer to abstractions, while their physical observations are always average values over a small region of space of volume δV. Depending on the measuring instrument, the size of the volume may be reduced up to a point. However, the field strengths of waves of wavelength much less than δl (the one-dimensional extension of the region) will not be detected. By diminishing the size of δV, however, a contradiction with the particle picture seemingly arises.

As, in a three-dimensional case, the energy and momentum of the measured wave are given by

$$E = \delta V \frac{1}{8\pi} \left(\vec{X}^2 + \vec{Y}^2 \right), \qquad \vec{P} = \delta V \frac{1}{4\pi c} \vec{Y} \times \vec{X},$$

where the values $\vec{X}$ and $\vec{Y}$ are the field strengths in both axes perpendicular to velocity, the amplitudes could be measured in arbitrary accuracy by diminishing δV. However, we know for quantization of radiation that the values are made of finite packets of magnitude $E_n = hv$ and $\vec{P}_n = hv/c$. This leads to uncertainty of precisely the

[21] The Compton scattering has momentum of magnitude $h\ /\ p$.

magnitude of the packets as the minimum threshold for the volume. The values $\vec{X}$ and $\vec{Y}$ have thus an uncertainty relation, which produces uncertainties for E and $\vec{P}$ by:

$$\Delta E = \frac{hc}{\delta l}, \qquad \Delta \vec{P} = \frac{h}{\delta l}.$$

Depending on the distributions and strengths of the fields, the expectation values for the amplitudes $\vec{X}$ and $\vec{Y}$ can be zero as well. Thus, the displacements $\Delta\vec{X}$ and $\Delta\vec{Y}$ have to themselves bring about the aforementioned uncertainty for energy and momentum. From this we get the relation:

$$\Delta\vec{X}\Delta\vec{Y} \geq \frac{hc}{\delta V \delta l} = \frac{hc}{(\delta l)^4}$$

Although this treatment considers electromagnetic waves and their Maxwell equations, the same relations hold equivalently for de Broglie matter waves. (Heisenberg 1930, 48–51.)

II.II.III.III. METHODOLOGICAL REMARKS AND DISCUSSION

The *Copenhagen spirit* is intimately related, as mentioned before, to the early discussion of quantum mechanics with methodological principles and the philosophy of science. At the very centre of this discussion is the concept of wave-particle duality. The idea of duality in matter first arose in de Broglie's seminal work regarding matter waves in 1924. The *de Broglie hypothesis* was proven to be correct, but the physical nature of the wave and particle pictures was left unclear, as they were initially seen as contradictory at the fundamental level of nature. As discussed in this chapter, the contradiction arises at the limit of accuracy determined by the uncertainty relations because it is not consistent for physical entities to simultaneously be described by wave- and particle pictures.[22] However, as a solution, de Broglie proposed the idea of a *pilot wave* in 1927, which, roughly put, suggests that a wave is associated with the particle, as a separate entity, guiding the motion of the particle (hence the title). The pilot wave theory provides a heuristic basis for the more advanced de Broglie-Bohm interpretation, which will be discussed in greater detail later.

[22] To clarify this somewhat, one can, for example, think of a ball at the end of a spring, undergoing sinusoidal motion. It is clear enough that we can observe a physical body in wavelike motion, but the physical natures of both the body and its motion are well-defined and separate notions. No such distinction can be made for electrons, as the particle *cannot* be localized without destroying other knowledge of it (such as momentum).

For Heisenberg, the duality problem requires no such solutions, but a well-defined understanding of the limits for both, which have been discussed above. Heisenberg (1930, 47) writes:

> After a critique of the wave concept has been added to that of the particle concept all contradictions between the two disappear – provided only that due regard is paid to the limits of applicability of the two pictures.

This statement is correct for applications of physics: all contradictions can be avoided as long as the lower limit of accuracy is included.[23] However, early on, this attitude was not adopted by all; one of the most famous examples is the Bohr-Einstein debates of 1927–1935. In the wake of the "quantum revolution", Einstein was dissatisfied with the epistemology of the *Copenhagen spirit*. This led to a prolonged debate between Einstein and Bohr regarding the epistemic and methodological constraints of quantum theory.[24]

For Einstein, the primary task of physics was to give a detailed account of any physical process, which he strongly felt quantum mechanics did not achieve. This led him to call the theory incomplete. As was already established by 1927, the uncertainty relation was ascertained as an unavoidable and fundamental attribute of all formalisms of quantum theory. Because the uncertainty principle restricts a causal[25] account of particle systems in spacetime, Einstein sought, by means of thought experiments, to find alternative ways to get past the limitations of the principle. In his reply to Bohr's account, Einstein states his general attitude towards the epistemology of quantum theory:

> What does not satisfy me in that theory, from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation). Whenever the positivistically inclined modern physicist hears such a formulation his reaction is that of a pitying smile. He says to himself: "there we have the naked formulation of a metaphysical prejudice, empty of content, a prejudice, moreover, the conquest of which constitutes the major epistemological achievement of physicists within the last quarter-century. Has any man ever perceived a 'real physical situation'? How is it possible that a reasonable person could today still believe that he can refute our essential knowledge and understanding by drawing up such a bloodless ghost?" (Schlipp et. al. 1949, 667.)

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[23] For instance, the uncertainty principle sets the lower limit for the size and density of semiconductors.
[24] The content of the debates is documented in Bohr's report in Einstein's volume of the Library of Living Philosophers (Schlipp et al. 1949) and in Zurek & Wheeler (eds.) 1983.
[25] For physicists widely and in general, the notion of causality is used in a quite restricted sense. Here, as "causal", only complete and certain accounts of both momentum and position at all points of spacetime apply. The notion itself will be discussed in more depth later.

First of Einstein's thought experiments was a modification to the already familiar single and double slit experiments. The diaphragm, which contains the slit the electrons are fired through, could be made to move freely with an instrument measuring its movement. Thus, the momentum transferred to the incoming wave packet could be measured by measuring the dislocation of the diaphragm, while knowing that the electron's position at that time is within the constraints of the slit. However, Bohr pointed out that problems would follow for the measurement of the diaphragm. If the momentum of the diaphragm is measured with great accuracy, its position will be unknown due to the uncertainty principle. And, naturally, if the diaphragm is fixed, no momentum measurement (to obtain information of the particle's momentum) can be executed.

Heisenberg points this out as well in "*Physical Principles"* as he discusses an experiment for detecting an electron passing through a cone of photons of a microscope. If the microscope could move, the momentum of the incoming photon could be measured, but (Heisenberg 1930, 22):

> […]this does not circumvent the uncertainty relation, for it immediately raises questions of the position of the microscope, and its position and momentum will also be found to be subject to equation [$\Delta p_x \Delta x \sim h$]. The position of the microscope need not be considered if the electron and a fixed scale be simultaneously observed through the moving microscope, and this seems to afford an escape from the uncertainty principle. But an observation then requires the simultaneous passage of at least two light quanta through the microscope to the observer – one from the electron and one from the scale – and a measurement of the recoil of the microscope is no longer sufficient to determine the direction of the light scattered by the electron. And so on *ad infinitum*.

Another challenge was presented by Einstein at the sixth Solvay conference in 1930. There he proposed that the mass-energy relations of relativity theory ($E = mc^2$) could be used to circumvent the uncertainty principle. The argument is as follows: if a box, containing radiation, and having a shutter attached to a clock of extreme accuracy which controls the opening and closing of the shutter, then the shutter could be applied to release a single photon from the box. If the box were weighed on an accurate scale immediately before and after the action of the shutter, both the location (at the shutter) and the momentum (from the mass difference of the box) of the photon could be known for a point of time of accuracy in contradiction with the uncertainty relation. The system, as presented by Bohr (1949), is of the kind pictured:

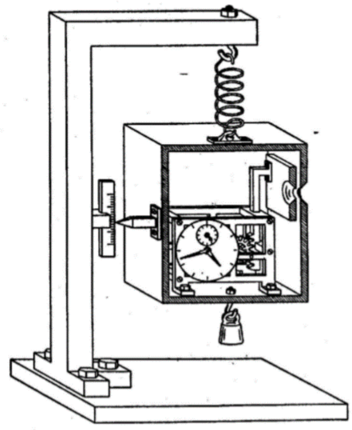

Figure 2: The Einstein particle-in-a-box experiment (Schlipp et. al. 1949, 227)

The scale function in the picture is performed by the system of a weak spring and a measuring scale with a corresponding pointer. The counterargument to this thought experiment, as presented by Bohr, is the following: to obtain greater accuracy from the mass measurement, one must also allow for a greater time interval for the measuring instrument (in this case, spring) to balance. Moreover, due to the equivalent time dilation effects of acceleration and gravity, a clock experiencing an acceleration due to the displacement of the box will have a differing reading when compared to the rest state. The difference in reading is in accordance to the time interval T required for the balancing of the scale by an amount of ΔT. The time dilation effect is then given by

$$\frac{\Delta T}{T} = \frac{1}{c^2} g \Delta q,$$

where g is the gravitational constant and Δq the displacement of the scale reading. After the weighing procedure there will be an uncertainty in the clock reading of amount

$$\Delta T > \frac{h}{c^2 \Delta m},$$

and combining this with $E = mc^2$ we get the uncertainty relation between time and energy (Schlipp et. al., 228):

$$\Delta T \Delta E > h.$$

The particular experiments in the examples above have the same general property: the measuring instrument cannot be separated from the object of experimentation. This is a more accurate way of stating the uncertainty principle than that the measuring device always *disturbs* the object. Because there is no way of meaningfully speaking about experimental results without the instruments carrying out the observations, it has to be accepted that whatever instruments that are used cannot be distinguished from their targets in terms of physical phenomena. This matter of phrasing was recognized as important by Bohr, as he stated (Schlipp et. al. 1949, 237):

> In this connection I warned especially against phrases, often found in the physical literature, such as disturbing of phenomena by observation or “creating physical attributes to atomic objects by measurements”. Such phrases, which may serve to remind of the apparent paradoxes in quantum theory, are at the same time apt to cause confusion, since words like “phenomena” and “observations”, just as “attributes and “measurements”, are used in a way hardly compatible with common language and practical definition.
>
> As a more appropriate way of expression, I advocated the application of the word “phenomenon” exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement.

However, even with the above specification, the discussion regarding these experiments might still give rise to a confusion of quantum theory: such that the uncertainty relations were caused by the introduction of experimental instruments. In order to avoid such a confusion, it is best to sternly emphasize the role of the uncertainty relations as the fundamental notion of quantum theory. As discussed, the formalization of quantum theory under matrix mechanics *rests on the axiom* of canonical commutation relations; if they did not hold, neither would the theory work. To shine as much light onto the situation as possible, recall the Hamiltonian equation of motion:

$$\frac{dA}{dt} = i\hbar[H, A] + \frac{\partial A}{\partial t},$$

which is a canonical commutation relation directly encompassing the uncertainty. Mathematically, the derivation of the equation involves assigning operators A for an observable a (e.g. position) and E for energy. The commutator $i\hbar[H, A]$ is then an expression of the Schrödinger equation. It trivially expands to

$$i\hbar(HA - AH),$$

which, if the order of operation were arbitrary for all H and A, would result to zero, signifying no action in all possible cases. Thus, if the uncertainty principle did not hold, quantum mechanics would be rendered useless.

The situation is no different for other (physically equivalent) pictures, i.e. wave mechanics. As stated by Heisenberg (e.g. in his 1930, 13–19), the uncertainty *necessarily follows* from the wave-particle duality. Recall that a particle has to be treated as a wave packet, and that amplitudes of a wave cannot be measured in an arbitrarily small region due to quantization. Therefore, the uncertainty relations lie under all properties of quantum-mechanical systems. A perhaps more rhetorically effective, and still as accurate, way of stating the same fact is: either the uncertainty principle holds, and atoms are stable (due to quantization of orbits), or atoms are not stable, given that their orbits would not be quantized (it's perhaps best to emphasize that this by itself does not provide an explanation for the lowest orbit, just that without it quantum theory would fall apart).

One further way of illustrating this unavoidability is to consider once again the diaphragm and the photographic plate. The diaphragm in this case has two slits, and the photographic plate once again receives the electrons fired through the slits. The experiment is conducted so that only one electron is fired at a time. Then, by repeated iterations, it is confirmed that single electrons obey the diffraction pattern caused by the colliding wavefronts incoming from both slits. For any *epistemic*[26] purpose, the only way to model the phenomena that is restricted to observed phenomena omits histories of the electron. The consequence of this is that the mathematical structure of such a model inevitably gives rise to the notions of 'superposition' and 'collapse', if such were to be discussed. This makes the set of uncertainty relations a kind of a physical principle, because there is no way to derive from observations a concept that would track a single path for the electron.[27][28]

Regardless, these thought experiments were not the last objections raised to the *Copenhagen spirit.* The third important challenge was presented by Einstein, Podolsky and Rosen in their 1935 paper "*Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?*". The paper included a property between two particles originating from the same system that came to be called the Einstein-Podolsky-Rosen paradox, or more briefly, the EPR paradox. To understand the paradox better, a quick look to many particle quantum mechanics is required.

The fundamentals of many-particle mechanics are consistent with the quantum-mechanical formalism of single-particle systems. The state vector $|\psi\rangle$ is taken as the

[26] Typically, objections to statements such as these are ontological (i.e. argumentation beyond empirical observation) – which will be discussed shortly.
[27] For further discussion, see e.g. Busch, Heinonen and Lahti (2006).
[28] The idea that this is *in principle* impossible is refuted by the proponents of the de Broglie-Bohm interpretation, and it is indeed possible to simulate the individual electron tracks as will be shown in the next chapter. However, all agree that no measurement exists (as of yet, at the very least) for the individual paths.

general description of the state of any system. Any single particle is associated with the Hilbert space $\mathcal{H}$, which describes the abstract space elements of which correspond to the possible values of the system. To describe a system of two particles with the state vector $|\psi\rangle$, we begin by denoting the combined product space of its subspaces $\mathcal{H}_1$ and $\mathcal{H}_2$ by:

$$\mathcal{H}_{12} = \mathcal{H}_1 \otimes \mathcal{H}_2$$

From each pair of state vectors, say,

$$|\psi\rangle^{(1)} \in \mathcal{H}_1 \text{ and } |\psi\rangle^{(2)} \in \mathcal{H}_2$$

we can construct the combined state vector as a direct product state:

$$|\psi, \phi\rangle \equiv |\psi\rangle^{(1)} \otimes |\psi\rangle^{(2)} = |\psi\rangle^{(1)}|\psi\rangle^{(2)} \in \mathcal{H}_{12}$$

This state, as it is a direct product state, can be divided (i.e. factored) into its respective substates (i.e. the states of individual particles). The state space $\mathcal{H}_{12}$ consists of all direct product states of $\mathcal{H}_1$ and $\mathcal{H}_2$ as well as all possible linear combinations of these. As an illumination, the Schrödinger equation of *N* particles can be constructed straightforwardly as:

$$i\hbar \frac{d}{dt}|\psi\rangle = H|\psi\rangle$$

$$\Rightarrow H = \sum_{a=1}^{N} \frac{P_a^2}{2m_a} + V(\bar{R}_N),$$

where $\bar{R}_N$ is simply the position vector series of *N* particles in the state Hilbert space $\mathcal{H}_N$.

However, the combined state space can also include states which are not direct product states – states that cannot be factored into states of separate subspaces. These states are called *entangled states*, which can only be represented in the combined space. In physical terms, this means that if a system of two or more particles is in such a state, the state of a single particle cannot be represented by a consideration of the single particle (of that system) alone. Entangled states are described by the formal definition (for pure states[29]):

$$\nexists\left\{|\psi\rangle^{(1)} \in \mathcal{H}_1, |\psi\rangle^{(2)} \in \mathcal{H}_2\right\} \ \ such\ that \ \ |\psi\rangle = |\psi\rangle^{(1)} \otimes |\psi\rangle^{(2)}$$

The above definition is a negative one[30]; it says that whenever there aren't direct product states that can be factored into these two subspaces, the state is entangled. To be emphasized here is that the volume of direct product states compared to entangled states diminishes as the complexity of the system increases, which is to say that separable states are generally a special set (see figure 3).

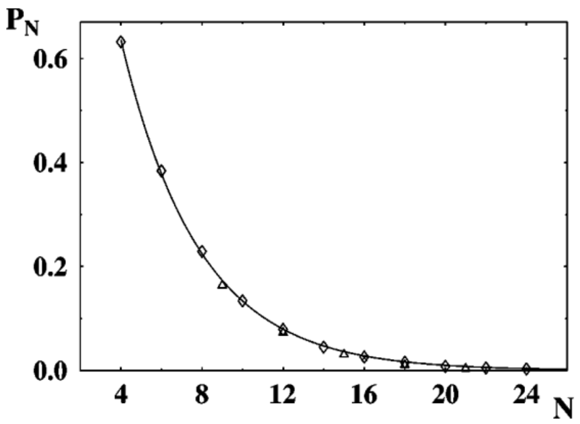

Figure 3: Graph of the volume of separable states as a function of the number of subsystems (Zyczkowski, Horodecki, Sanpera and Lewenstein, 1998.)

The EPR argument considers a bipartite (two-particle) system, in which the particles interact in the interval $t + \Delta t = T$, and are in no interaction in times $> T$. The states of both systems before t are known. Now, assume that we are experimentally interested of only one physical quantity, for example A, and its respective eigenvalues a_n. The state of neither system after T cannot be calculated, but it has to be subjected to further measurements, causing the wave function to collapse. Mathematically, the state after T but before observation can be expressed as the infinite series

$$|\psi\rangle = \sum_{N=1}^{\infty} \psi_N(x_2) u_N(x_1),$$

[29] This is the definition of *pure entangled states*. Pure states are a property of a system isolated from its surroundings. This is an idealization, since this is impossible for almost all quantum systems. In the majority of cases we consider mixed states where the formalism includes the traces of the systems environment. For the purposes of simplicity, however, we consider pure states here whenever possible.
[30] Naturally, an equivalent positive definition exists.

where the terms $\psi_N(x_2)$ function as coefficients for the expansion of ψ into orthogonal functions $u_N(x_1)$, and generally the two wavefunctions represent the states of respective particles. The x describe the range of variables of the wavefunctions. After observation of *A*, the superposition of the quantity collapses into a definite state a_k, and the two wavefunctions are represented by

$$\psi_k(x_2)u_k(x_1).$$

Suppose now that instead of *A*, the quantity *B* should be measured instead (in an otherwise identical situation). Because the observed eigenvalues b_n are similarly the result of their respective wavefunctions, the measurement causes a wavefunction collapse into different definite functions, say

$$\phi_r(x_2)\varphi_r(x_1).$$

To be clear, this relation holds when just one of the two systems is measured. Now, suppose that two consequential observations are executed for one system, first for quantity *A* and then *B*. As long as the two systems no longer interact, this should mean that the other system is now described by two different wavefunctions. Furthermore, suppose that these two quantities were, for example, the position and momentum operators $\hat{X}$ and $\hat{P}$ which should not commute. For Einstein, Podolsky and Rosen, this seemed to provide a way of defining a physical reality to the elements of both $\hat{X}$ and $\hat{P}$, establishing that quantum mechanics is not a complete description of physical reality. (Einstein, Podolsky and Rosen 1935.)

In the interest of time, the historical discussion regarding the paradox between Einstein and Bohr is skipped over, and instead the conclusive reason of why this does not work is shown. The EPR argument supposes a postulate of many-particle systems that can be called the “local hidden variable model” (LHVM). This supposition is that after interaction, the two or more particles involved in the interaction assume hidden variables that determine the states of the collapsed wavefunctions. Because interaction can supposedly only be local, the information gained from the two measurements of the first particle also provides information of the second particle without disturbing it.

The problem here is that the entangled states discussed earlier are defined by their direct violation of LHVM, as the definition of entanglement is the impossibility of factorization of the quantum state to its subsystems. There are two other options: if the states are separable, they are (i) either non-entangled states without local interaction (where no information of other systems relevant to this situation is attainable by measurements of one), or (ii) non-entangled states with local interaction with each other, where the measurement is once again a part of the total system. Naturally, this information regarding entangled states was not available in 1935. The contemporary understanding was developed first by Bell in his (1964) where a set of inequalities, later named the Bell inequalities, were derived from the LHVM assumption. These inequalities were shown to be inconsistent with the predictions of quantum mechanics. Later, the predictions in question were shown to be empirically correct by Clauser and Freedman in 1972 and Aspect, Dalibard and Roger in 1984, killing the idea of local hidden variables.

One question of interest since the discovery of entanglement has been the nature of interaction between particles in entanglement but not in local interaction, as it raises questions about violations of relativity theory and conservation of energy. However, it is commonly agreed that no signal (defined as transmitting and receiving information) is transmitted between the subsystems, as causally manipulating the states of the systems is impossible.[31] (E.g. Popescu and Rohrlich 1997, Peres and Terno 2004.)

As a closing remark regarding the disagreements between Einstein and "those of the Copenhagen spirit", the nature of the argumentation was categorically epistemological or methodological, and thus strictly in the domain of physics as a first-order scientific enterprise. Einstein's pursuit was to use thought experiments to come up with experimental arrangements that could circumvent the uncertainty principle. Had these worked, the process would have been a part of development of physics by the means of novel empirical discoveries. The nature of argumentation for *interpretations* that were to follow, as the reader will come to see, is different – and generally better classifiable under the category of *philosophy of physics*.

[31] This is the content of the so-called "no-communication theorem". However, whether a signal is transmitted between entangled systems can also be argued to be a matter of definition. If interaction of any kind, a category that includes entanglement, is defined to necessitate signalling, then, conceptually, signals are transmitted between entangled systems. However, this is a trivial point to make for all practical purposes, as the possibility of communication by means of entanglement remains barren. For further discussion, see Walleczek & Grössing 2016.

III. THE DE BROGLIE-BOHM INTERPRETATION

III.I. EARLY DEVELOPMENT

The discovery of matter waves in 1924 by Louis de Broglie led him to sketch out a model of the quantum system where a separation between a wave and a particle is maintained. This was to be called the “pilot wave theory”. Its core property is that at any time, a particle is accompanied by a guiding wave.

Initially, de Broglie published the idea of the pilot wave in his 1927 paper *“‘Wave mechanics and the atomic structure of matter and of radiation”*. However, the pilot wave was presented by de Broglie as a provisional theory as an alternative to another of his postulates. To summarize the paper, de Broglie begins by treating particles described by the wave function as singularities in a wave field $u(\bar{x},t)$. The motion of the particle is given by a Klein-Gordon equation of a wave *u*:

$$\nabla^2 u - \frac{1}{C^2}\frac{\partial^2 u}{\partial t^2} = \frac{4\pi^2 v_0^2}{c^2}u,$$

which is a variant of the Schrödinger equation describing the relativistic motion of bosons. The solution de Broglie proposes is of the form

$$u(\bar{x},t) = f(\bar{x}-\bar{v}t)\, cos\frac{2\pi}{h}\phi(\bar{x},t),$$

where *f* denotes the amplitude of the wave. At the location $\bar{x} = \bar{v}t$ the amplitude is singular (i.e. at this point we find a discontinuity in the wave). The phase of the wave, $\phi(\bar{x},t)$, is given by

$$\phi(\bar{x},t) = \frac{hv_0}{\sqrt{1-\frac{v^2}{c^1}}}\left(t - \frac{\bar{v}\cdot\bar{x}}{c^2}\right)$$

which is equivalent with the Hamiltonian representation of the Schrödinger equation. In other words, here we find the system described in terms of phase and velocity instead of total energies. Now, de Broglie argues that for an *ensemble* of particles, the preceding can be represented as a *continuous* equation

$$\psi(\bar{x},t) = a\, cos\frac{2\pi}{h}\phi(\bar{x},t),$$

where *a* remains constant. Having given these for the cases of free particles, de Broglie considers particles in an external potential, where the Klein-Gordon equation is expressed in a complex form accordingly to the Schrödinger equation. Here, what needs to be solved is the extension of the phase ϕ to cases where the external potential is non-zero. Solving for the associated differential equation, and generalizing the phase as

$$\phi(\bar{x},t) = Et - \phi_1(\bar{x}),$$

ϕ_1 being the time-independent phase function, one of the solutions then becomes

$$\hbar\frac{\boxdot f}{f} = (\nabla\phi_1) - \frac{1}{c^2}(E-V)^2 + m_0^2c^2,$$

where

$$\boxdot = \nabla^2 - \frac{1}{c^2}\frac{\partial^2}{\partial t^2}.$$

What needs to be accounted for here is the relation between the terms f and ϕ_1, i.e. the relation between the velocity of the singularity and the phase of the wave. In the classical limit, the directions of the velocity and the phase vector are equal. The "quantum effects" demonstrated by the uncertainty relations arise when the term $\boxdot f$ is non-zero; de Broglie postulates that the equality remains unaltered in these cases as well.

After this treatment of singularities in an external potential, de Broglie proceeds to discuss continuous waves in the similar case. Here, de Broglie derives from the associated motion equation the solution

$$\hbar^2\frac{\nabla^2 a}{a} = (\nabla\phi_1')^2 - \frac{1}{c^2}(E-V) + m_0^2c^2,$$

which shares the same form with the singular equation. Of these two, de Broglie proposes that the phase functions ϕ_1 and ϕ_1' are *always* equal, leading into a so-called "double solution": equal solutions to the Klein-Gordon equation describing both singular and continuous waves. To be stressed here is that two critical *assumptions* have been made: one of the generality of the relation between the vectors of velocity and phase inside a classical limit, and one of the equality of phases of singular and continuous waves. Generalizing the solutions to a charge in a time-dependent electromagnetic field, with these assumptions, de Broglie presents the general expression for the velocity of the particle:

$$\bar{v} = -c^2 \frac{\nabla\phi + \frac{e}{c}\bar{A}}{\dot{\phi} - e\vartheta}.$$

Building from this, de Broglie proceeds onwards to give similar accounts by the double solution of cases with an external potential. Summarizing the steps that were to follow, the end game was to show that there exists a solution for the 6-dimensional phase function of two particles $\phi(\bar{x}_1, \bar{x}_2)$ such that a corresponding (generally well-defined) wave function $\psi(\bar{x}_1, \bar{x}_2, t)$ is satisfied. Due to an error in de Broglie's argumentation and for the sake of brevity this will not be discussed further.

However, in the latter part of the paper, de Broglie proposes an alternative approach to the above velocity equation. Instead of vindicating the expression of velocity by attempting to find a solution to the corresponding phase functions (invoking the double solution), the phase can be treated as a physically separate entity from the particle. Thus, the wave function, presented by de Broglie in the polar form

$$\psi(\bar{x}, t) = a(\bar{x}, t)e^{(i/\hbar)\phi(\bar{x},t)},$$

describes a continuous wave (described in the phase function) associated with a material point (described the position vector of the amplitude). This idea was extended by de Broglie at the 1927 Solvay conference, where he provided an account of the pilot wave theory for a many-body system. After giving the wave function and the phase for one particle exactly as in his recent paper, de Broglie went on to sketch the velocities of *N* particles by

$$\bar{v}_k = -\frac{1}{m_k}\nabla_k\phi,$$

where the probability for the system to *be* in a volume element $d\tau$ is

$$\pi\, d\tau = Ca^2\, d\tau,$$

which is to say that the amplitude of the wave in a given volume element dictates the probability. As velocity is the fundamental variable for de Broglie's theory, he generalized the classical Hamilton-Jacobi equation for this purpose. The particle trajectories are determined (causally)[32] by initial conditions of the system; thus de

[32] By causality, de Broglie means a fully well-defined causation from initial conditions to positions (i.e. a relationship beyond the uncertainty principle).

Broglie argued that determinism should not be abandoned. (Bacciagaluppi & Valentini 2009, 69—76).

De Broglie's presentation in 1927 is among the first times a differing conceptual interpretation of quantum mechanics was proposed.[33] And later the "quantum Hamilton-Jacobi" became central to the de Broglie-Bohm interpretation. Although de Broglie's ideas were quickly rejected by the physics community at the time as speculative and irreconcilable with experimentation (Bacciagaluppi & Valentini 2009, 233—242), very similar ideas were (independently) brought up by David Bohm some twenty years later. To be emphasized, in any case, is the notion of *interpreting* quantum mechanics which became fundamental for the development of modern philosophy of physics. Unlike Einstein, de Broglie did not attempt to come up with experimental arrangements but rather derived and postulated mathematical structures which were then to be interpreted.[34]

III.II. THE ONTOLOGICAL INTERPRETATION

As mentioned, similar ideas that were first presented by de Broglie were later independently picked up by David Bohm, leading to the publication of his 1952 article *"A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden' Variables"*. The ideas in the paper were further developed in his 1957 book *"Causality and Chance in Modern Physics"*. Later on Bohm collaborated with physicist Basil Hiley, resulting in their final book *"The Undivided Universe"* (1993). The theory underwent some changes in the decades in between, but for the purposes of this thesis, only the final theory, which Bohm and Hiley dubbed the "ontological interpretation of quantum theory", will be discussed. The basics of the ontological interpretation are given a great deal of space here – this is because scrutinizing its properties is the point of this thesis.[35]

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[33] Madelung (1926) proposed an alternative hydrodynamical formulation to the Schrödinger equation the year before.

[34] The possibility of an experiment, distinct from the predictions made by standard quantum mechanics, is one of the central points of contention between proponents of different interpretations and already introduced by Bohm in his 1952 paper (Bohm 1952a, 179). This theme will be discussed further in III.III.

[35] There has, naturally, been further discussion regarding the ontological interpretation and "Bohmian mechanics" in the years after 1993. There are also differences between the proponents of the de Broglie-Bohm interpretation (see e.g. Holland 2014). The core ideas for the interpretation that are relevant for this thesis, however, remain sufficiently unchanged across time and authors.

For single-particle systems, Bohm and Hiley formulate the WKB approximation[36] of the classical limit of quantum mechanics by writing the standard wave function in polar form

$$\psi = R\mathrm{e}^{\left(\frac{iS}{\hbar}\right)},$$

where *R* and *S* are real functions. This is then inserted into the Schrödinger equation. The solutions for this are

$$\frac{\partial S}{\partial t} + \frac{\nabla S}{2m} + V - \frac{\hbar^2 \nabla^2 R}{2mR} = 0$$

and

$$\frac{\partial R^2}{\partial t} + \nabla \cdot \left(R^2 \frac{\nabla S}{m}\right) = 0$$

of which the latter is the continuum equation (which ensures the conservation of probability) and the former is what Bohm and Hiley call “the quantum Hamilton-Jacobi” equation. (Bohm & Hiley 1993, 28—30.) The key difference here to the standard WKB approximation in the classical limit is that the last term which is customarily neglected is instead preserved. Bohm & Hiley call this the *quantum potential*:

$$Q = \frac{\hbar^2 \nabla^2 R}{2mR}.$$

Here, the term *S* is the phase of the system and $R^2 = \rho$ (i.e. the probability density of a given point – as well as the amplitude of the quantum field). The quantum potential is a key concept in Bohm’s and Hiley’s thinking. It differs from the “classical” potential, *V*, because its effect to a given particle is mediated non-locally. Bohm and Hiley write:

> [T]he quantum potential is not changed when we multiply the field ψ by an arbitrary constant. (This is because ψ appears both in the numerator and the denominator of *Q*.) This means that the effect of the quantum potential is independent of the strength (i.e. the intensity) of the quantum field but depends only on its *form*. By contrast, classical waves, which act mechanically (i.e. to transfer energy and momentum, for example, to push a floating object), always produce effects that are more or less proportional to the wave. (Bohm & Hiley 1993, 31.)

[36] The WKB approximation (initialism for Wentzel—Kramers—Brillouin) is a method of approximating linear differential equations such as the Schrödinger equation.

With the concept of quantum potential, Bohm and Hiley flesh out their *ontology* for the quantum-mechanical system. The four main points are:

OI1. The electron is, at all times, a particle, with a well-defined position which is causally determined.
OI2. Its equation of motion is determined by Q as well as V.
OI3. The wave function describes a *quantum field* which accompanies the particle at all times.
OI4. The way the electron is coordinated is described by a *guidance equation*.

The guidance equation gives the electron's velocity in configuration space in respect to time-evolution. It is written as

$$\vec{v}(\vec{r},t) = \frac{\nabla S(\vec{r},t)}{m},$$

which is the evolution of phase in respect to time and position (Bohm 1952).

This approach is similar to de Broglie's alternative concept of the pilot wave. De Broglie's idea was to avoid the issues relating to the (non-)correspondence of respective velocity and phase functions by treating the wave and the "material point" as distinct entities, and, in effect, that is exactly what's being done here.

Where Bohm and Hiley go further is fleshing out the mathematics and ontological implications based on this concept. Importantly, the causal relation between the quantum potential and the particle needs to be explained. This is done by introducing another new concept, *active information.* Basically, the quantum potential feeds a given particle instructions for operation, such as those determining position and momentum. The set of these instructions is the active information. It differs from the common usage of the term 'information' insofar that it does not need to refer to subjectivity, evidence or knowledge. Instead, it is meant to be taken in the literal sense 'in-form' – meaning that the potential gives the particles' actions their form. The quantum potential, then, is not to be understood as a mechanical force:

> Although equation (3.8.)[37] may look like a classical law implying pushing or pulling by the quantum potential, this would not be understandable because a very weak field can produce the full effect which depends only on the form of the wave. […] So the ability to do work does not originate in the quantum field, but must have some other origin[.] (Bohm & Hiley 1993, 37.)

[37] $m\frac{dv}{dt} = -\nabla(V) - \nabla(Q)$

The ‘active’ side comes from the idea that the information encompassed by the quantum potential becomes *actually active* when a particle is guided by this information and *potentially active* wherever the quantum potential is non-zero. How, then, can this information be processed? Bohm and Hiley suggest that active information implies some sort of inherent faculties for a particle, or a “complex inner structure”:

> The fact that the particle is moving under its own energy, but being guided by the information in the quantum field, suggests that an electron or any other elementary particle has a complex and subtle inner structure (e.g. perhaps even comparable to that of a radio) (Bohm & Hiley 1993, 37).

A practical example of these concepts in effect is the already familiar double slit experiment. In the experiment electrons are fired through a diaphragm with two narrow slits for the electrons to pass through, and then captured on a photographic screen. The electrons are selected so that all are incident to one of the two slits. Here, Bohm and Hiley argue that the *quantum field* (ψ) actually precedes the particles through the slits and provides to the pool of active information available to the particles. Now, the trajectories for all particles are pre-determined by the quantum potential and at no point is their location or momentum not well-defined.

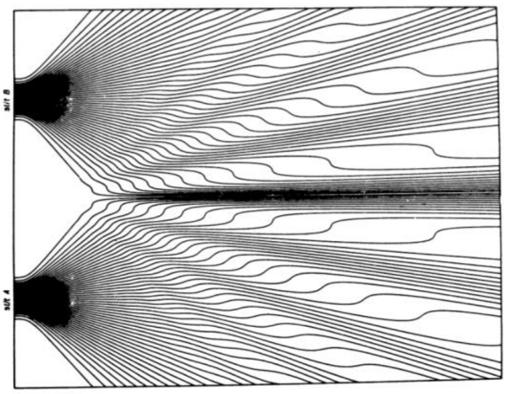

Figure 4: Proposed trajectories in a double-slit experiment (Bohm & Hiley, 1993, 33.)

Respectively, the same mathematical treatment is given to many-body systems. For a system of two particles, writing the wave function in the polar form and solving the associated Schrödinger equation we get the Hamilton-Jacobi equation with the associated quantum potential:

$$\frac{\partial S}{\partial t} + \frac{(\nabla_1 S)^2}{2m} + \frac{(\nabla_2 S)^2}{2m} + V + Q = 0$$

$$Q = -\frac{\hbar}{2m}\frac{(\nabla_1^2 + \nabla_2^2)R}{R}$$

with the continuum equation

$$\frac{\partial P}{\partial t} + \nabla_1 \cdot \left(\frac{P\nabla_1 S}{m}\right) + \nabla_2 \cdot \left(\frac{P\nabla_2 S}{m}\right) = 0.$$

The causal picture here is an extension of the one-body systems, but it also contains the particles' mutual interaction. As discussed before, an important property of many-particle quantum mechanics is entangled states. For Bohm and Hiley, this entails *non-local* interaction.[38] In line with the contemporary understanding of many-body quantum mechanics, they note that entangled states make the decomposition of the system (i.e. separable equations of it) impossible already at the level of a hydrogen atom: "it is impossible to find a single pre-assigned function of *r,* which would simultaneously represent the interaction of electron and proton in both *s*- and *p*-states" (Bohm & Hiley 1993, 58).

In this picture, the phase *S* and the quantum potential *Q* determine how the wave function affects particles in a given system. Particles then coordinate themselves according to a *common pool* of information, contained by the quantum field. If a system *can* be factorized to some number of subsystems (e.g. non-entangled states), these subsystems correspond to their independent pools of information. This pool is formally in configuration space (as is the case of formalisms of quantum mechanics in general), so it cannot be regarded as accessible in the three spatial dimensions – Bohm & Hiley (1993, 61) write:

> This is a further factor in addition to the form dependence of the activity of the field which leads us to consider the interpretation of this field as active information. The multidimensional nature of this field need not then be so mysterious, since information can be organized into as many sets of dimension as may be needed.

As illustration, Bohm and Hiley sketch the formulas associated with the double slit experiment where the diaphragm (of mass *M*) is treated as a part in a bipartite system. It has a single, constant mass coordinate *y*, where the movement of the particle (of mass

[38] To note: entanglement and non-locality are not the same thing, for there exist entangled states which agree with the LHVM model. However, entanglement is a necessary condition for non-local interaction. (See e.g. Werner, 1989.)

m) is mapped to the *x*-coordinate. Approximating $M>>m$, we get the quantum potential for the electron:

$$Q = -\frac{\hbar^2 \nabla_x^2 R(\vec{x} - \vec{y})}{2mR(\vec{x} - \vec{y})}$$

Because of the mass difference[39], the quantum potential only acts on the electron, but the two systems are regardless participants to the same pool of information determining the movement of the electron. Thus, there is non-local interaction between the diaphragm and the particle. This relationship is functionally the same in the traditional examples of non-locality, such as the entanglement of the spin-states of two particles, insofar as they are affected by a common quantum field and thus participants to common information. (Bohm & Hiley 1993, 56—71.)

III.II.I. RELATION TO THE BORN RULE

The wave function is a state-representation of a quantum system. For any given state, the standard interpretation is that it gives a *probability amplitude* for a given state to be found with given parameters. Thus, taking the square modulus we get the probability density at this point for a particle to be found at a given point in space. This is the Born rule:

$$|\psi(x,t)|^2 = \psi^*\psi = \rho(x,t).$$

Because the net probability must amount to one, we get the normalization condition:

$$\int_{-\infty}^{\infty} \psi^*\psi \, dx = 1.$$

So, the received view of the Born rule is that the wave function is a representation of *epistemic probability*. (Landsman 2009). By epistemic probability it is simply meant that the real observations made are congruent with the predictions entailed by the Born rule – but that nothing beyond that (frequentist) notion regarding the wave function can be stated. Bohm and Hiley disagree with this, stating that the relationship between $|\psi|^2$ and ρ is not necessary[40], but that in usual conditions the probability density only

[39] The diaphragm system can be thought to be held externally fixed to eliminate complications regarding its movement.

[40] To clarify: if the square of the absolute value of the wave function and the probability density *were* demonstrably different notions, the Born rule would not hold.

approaches, or even equals an equilibrium distribution which is represented by $|\psi|^2$. In other words, in this theory, a system may initially be in a state disagreeing with standard quantum mechanics, but then evolve to a higher-entropy state which is always described by $|\psi|^2$. This is, of course, a necessary step to make, for Bohm and Hiley explicitly state that ψ itself is a representation of a separate quantum field. Now, the term ρ determines the quantum potential in this respect. Bohm and Hiley write:

> This theory is formulated basically in terms of what Bell has called 'beables' rather than of 'observables'. These beables are assumed to have a reality that is independent of being observed or known in any other way. The observables therefore do not have a fundamental significance in our theory but rather are treated as statistical functions of the beables that are involved in what is currently called a measurement. (Bohm & Hiley 1993, 40—41.)

Empirically, no such cases are found where the relation $\rho \neq |\psi|^2$ arises (which could be the case at an early stage before entropic interactions). However, Bohm and Hiley briefly discuss the possibility of such an experiment.[41] (Bohm & Hiley 1993, 181—190.)

III.II.II. SYSTEMS AS INDEPENDENT OF OBSERVATION

To cash out the intended ontology, the description of these systems must be given in a way which does not rest on them being brought about by the observations alone. This means describing the process, including the instance of observation, as a total causal system. Bohm and Hiley give it their shot by giving an example of quantum tunnelling in one dimension. In quantum tunnelling, a wave packet is incident to a potential barrier of "height" *V*, at which point the wave packet is either reflected by the barrier, at some point of its lateral dimension, or transmitted through it. For singular wave packets, the process is seemingly undetermined (as is the case with e.g. where the electrons end up on the screen in the double-slit experiment) with some expectation values assigned for each possibility. The wave function is, then, different for wave packets in each of the three regions with separate complex coefficients for each of the functions:

incident from the left + reflected from the right:

$$\psi_1 = Ae^{ikx} + Be^{-ikx}$$

[41] It is also suggested by Valentini (2008) that such cases might be found, by means of inflationary cosmology, in the vicinity of the Big Bang.

transmitted from the left + reflected from the right:

$$\psi_2 = Ce^{qx} + De^{-qx}$$

transmitted from the left:

$$\psi_3 = Ee^{ikx}$$

In this case, as some of the particles are transmitted through the barrier, there is a small net velocity in the positive direction at the point where the incident and reflected wave packets overlap, with the standard form of an unbound wave packet in the region where only the transmitted wave packet is found:

$$v_1 = \frac{1}{i2m}\left(\frac{\psi^*\nabla\psi - \psi\nabla\psi^*}{\psi^*\psi}\right) \qquad\qquad v_3 = \frac{k}{m}$$

Bohm and Hiley argue that the transmission of a particle is causally determined by its initial position. Starting by modelling particles in the tunnelling case by mapping their trajectories from Gaussian[42] functions (figure 5), Bohm and Hiley conclude that only the particles in front of a given wave packet have a possibility of transmitting. However, most of the particles that penetrate the potential barrier are eventually reflected. This is caused, essentially, by the wave packet dividing into two distinct *channels*: the channel of transmission and that of reflection.

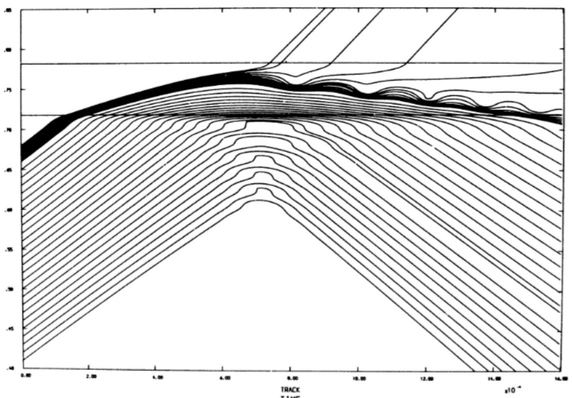

Figure 5: Proposed trajectories in quantum tunnelling (Bohm & Hiley 1993, 79.)

[42] The use of Gaussian functions in trajectory mapping is its own interesting subject with unfortunately little room in this thesis. The crux of the Bohmian approach is to calculate individual trajectories of particles in spreading of the Gaussian wave packet. In a simple free-particle case, combining the equations for the Gaussian wave function, the polar form of the wave function and expanding S we get

$$v(x,t) = u + \frac{(x-ut)bt}{(1+bt^2)}$$

where u is the initial velocity of the ensemble and $b = \hbar^2 \,/\, 4m^2\sigma_0^4$. Integrating the equation of velocity one gets the trajectories for N number of particles. This methodology is used in figures 4 and 5. For a good summary, see Kumar Pan 2010. For more on the subject of trajectories in phase space, see e.g. Martens 2008.

In this case, instead of calculating the probability of transmission, the same equation refers to the probability of being in the "transmission-channel". These channels do not overlap and remain distinct throughout the particle's trajectory from its initial position to observation. They also contain their respective sets of active information and coordinate particle behaviour accordingly. Here Bohm and Hiley also introduce the distinction between *active and inactive* information. Because it is possible to alter the system mid-process so that the channels are forced to overlap, Bohm and Hiley conclude that a given particle is also associated with an *empty channel* containing inactive information, with the potentiality of the information becoming active later on.[43] (Bohm & Hiley 1993, 78—82.)

This however raises a challenge: if these kinds of alterations make the channel-selection an open toss once again, it would seem that an observation is ultimately required to make it irreversible. Thus, this description seems to lie on the notion of observation after all. Bohm and Hiley argue that this is not the case, as this problem arises only in abstractions of a one-body model, whilst all the actual interactions involving multiple particles end up producing these irreversible effects.[44] (Bohm & Hiley 1993, 78—82.)

To avoid any notions of wave function collapse or corresponding ideas, also the instance of observation needs to be fleshed out in the ontological framework. Bohm and Hiley do this in a functionally similar way as they treated the diaphragm. That is, they treat the measuring instrument as a part of a many-body system. Defining the *n*-particle wave function as the standard series and introducing the measuring instrument as a wave packet of form $\phi_0(\vec{y})$, the combined wave function is given by:

$$\psi_i(\vec{x},\vec{y}\,) = \phi_0(\vec{y}) \sum_N C_N \psi_N(\vec{x}).$$

When the particles interact with the measuring instrument, the wave function evolves into:

[43] The idea of inactive channels of information also entails that there are corresponding "empty wave packets" which do not act upon the particles in any way. Bohm's and Hiley's reasoning for this lies on the conception of the wave function as a description of a "field of information" instead of a physical wave.
[44] An example of particle scattering in a 3D tunnelling situation is given to illustrate this argument in Bohm and Hiley (1993, 79—82). The main point is that during the interaction of wave packets, non-overlapping channels multiply and become distinct, which eliminates the possibility of future overlapping. While this seems to still beg the question of observation, it will not be specifically discussed here.

$$\psi(\vec{x},\vec{y},t) = \sum_N C_N \psi_N(\vec{x}) \phi_0(\vec{y} - \lambda O_N t),$$

where O_N are the eigenvalues of the operator O, with the respective eigenfunctions $\psi_N(\vec{x})$. In the duration of this interaction, Bohm and Hiley describe the same kind of overlapping of channels as was in the tunnelling case. After interaction, any overlap is eliminated, and the measuring instrument is forced to irreversibly “enter” a certain wave packet (i.e. enter a certain channel of information), in this case $\psi_m(\vec{x})\phi_m(\vec{y})$, with the corresponding probability for the instrument to be in that state corresponding to $|C_N|^2$. The system will then be described by the wave function (Bohm & Hiley 1993, 98):

$$\psi_f = \sum_N C_N \psi_N(\vec{x}) \phi_0(\vec{y} - \lambda O_N \Delta t).$$

Bohm and Hiley write:

> We may [...] indeed say that each of the possibilities $\psi_m(\vec{x})\phi_m(\vec{y})$ constitutes a kind of a channel. During the period of interaction with the quantum potential develops a structure of bifurcation points, such that apparatus particles initially on the trajectories leading to one side of these points enter, for example, the m:th channel, while the others do not. Eventually each particle enters one of the channels to the exclusion of all the others and thereafter stays in this channel. [...] The fact that the apparatus particle must enter one of the possible channels and stay there is thus what is behind the possibility of a set of clearly distinct results of a quantum measurement. (Bohm & Hiley 1993, 99.)

Thus, Bohm and Hiley argue that the notion of a “wave function collapse”, at least in a fundamental sense[45], is averted. To clarify, what is argued for here is that causal determinism is preserved – what happens in the interaction between the measurement device and the measured system is a loss of potential in the *unoccupied wave packets*, or, the *inactive channels*. Thus, well-definedness is conserved regardless of overlapping or channel-selection mid-process.

What does this mean in regard to the uncertainty principle? Bohm and Hiley naturally agree that the limitations imposed by the principle remain in place but refuse to accept that they entail that nothing under the effect of the uncertainty principle can be discussed (Bohm & Hiley 1993, 114). Instead, they liken such epistemological stances to “as if in the observation of the mature plant, we were said to be ‘measuring’ the

[45] Bohm and Hiley also write: “[W]e can say that everything has happened as if the overall wave function had ‘collapsed’ to one corresponding to the actual result obtained in the measurement. We emphasise, however, that in our treatment there is no actual collapse[.]”

properties of the seed" (ibid). As did Bohr, Bohm and Hiley recognize that the participatory nature of the observation event cannot be experimentally isolated from the observed system. However, they maintain, as their main contrast with Bohr, that the independent system should be discussed as well.

III.III. METHODOLOGICAL DISCUSSION AND CRITICISM

The de Broglie-Bohm interpretation is usually categorized as a hidden variable interpretation, although it bears little to no resemblance to the LHVM introduced in the EPR paper[46]. The reasoning for this is that the description of the particle's position along with non-local interaction with the quantum potential can be thought of as some sort of a "non-local hidden variable theory". Indeed, much of the discussion pertaining to the possibility of hidden variables in quantum mechanics have involved the de Broglie-Bohm interpretation in some way or another. Of note here is that to correctly recount the discussion, the de Broglie-Bohm interpretation is referred to as a hidden value interpretation, but the term is arguably ill-defined and loose (referring, with some caveats, to *anything* that makes a theory deterministic).[47]

All hidden variables in quantum mechanics were supposed to be proven impossible by John von Neumann in his 1932 book *"Mathematische Grundlagen der Quantenmechanik"*. In short, the proof goes as follows: take whichever two quantities, say, $\vec{R}$ and $\vec{S}$. In a statistical ensemble, a measurement can be made for both $\vec{R}$ and $\vec{S}$ in separate subsystems. Thus we get the definition of their combined expectation value:

$$\langle \vec{R} + \vec{S} \rangle = \langle \vec{R} \rangle + \langle \vec{S} \rangle$$

If the two quantities cannot be jointly measured (as is the case, of course, with position and momentum), the definition is implicit in the sense that it does not correspond to any measuring arrangement. Now, two assumptions must hold at all times when dealing with quantum theory. Firstly, for every physical quantity $\vec{R}$ there must be a corresponding Hermitian operator R in Hilbert space. Secondly, this correspondence must include addition relations. From this it can be deduced[48] that for any arbitrary

[46] It is important to separate the LHVM, which is conclusively proven to be false, from the wider usage of the term "hidden variables". It has been shown that locality cannot be preserved, but if hidden variables are taken to include causal theories of non-local interaction as well, no such demonstration exists.
[47] The notion was also rejected by Bohm & Hiley (1993, 2).
[48] It's impossible to go through the whole proof in full detail here.

physical quantity in any arbitrary ensemble of systems, say $\vec{R}$, its expectation value can be written as

$$\langle R \rangle = \mathrm{Tr}(UR),$$

where *U* is a Hermitian density operator characterizing the ensemble. From this it follows that whatever the choice of *U* and *R*, the statistical spread of *R* in the trace does not vanish. Because the whole point of hidden variables is to eliminate all statistical spread, it is concluded that they cannot be introduced into the formalism of quantum mechanics without breaking it apart.

The proof was widely accepted as the final word on hidden variables until Bell in his 1966 paper "*On the Problem of Hidden Variables in Quantum Mechanics"*. Shortly put, Bell's criticism was that von Neumann's proof lied on a false assumption regarding linear combinations of operators. For non-commuting operators, the combined eigenvalue *r+s* of the operator combination *R+S* is not a linear combination of *R* and *S* separately, and because of this, the proof does not exclude all hidden variables. More generally, it does not exclude any theory reproducing the empirical content of quantum mechanics in a different framework. However, the proof still successfully demonstrates that hidden variables cannot be included in a formalism which assigns all measurable physical quantities bijective operators in Hilbert space. In other words, the operator formalism would not work without statistical dispersion. This implies a limitation on *possible* quantum-physical theories: as long as an essential characteristic of the theory is the operator bijection as presented above, the spread cannot be eliminated.[49]

In any case, as far as interpretations go, hidden variables do not *exclude* correct empirical predictions in quantum mechanics, which is why it is possible for the de Broglie-Bohm interpretation to be non-contradictory. The interpretation of ψ as a "quantum field" in the context of the solution to the Schrödinger equation, in the words of Bernstein (2011), has nothing to do with the von Neumann proof.

A second challenge to any hidden variable interpretation of quantum theory is the Kochen-Specker theorem, which is a further development of the kinds of no-go-theorems that started with von Neumann's proof, with narrower focus on properties of

[49] It is argued that this was von Neumann's intention anyway, as opposed to giving some sort of conclusive proof of the impossibility of hidden variables in nature - while other views on the subject have simply been misunderstandings on behalf of both opponents and proponents of hidden variable theories. See Bub 2010.

quantum theory itself. Whereas the caveat in the von Neumann proof was that non-commutative operators were able to violate the theorem, the Kochen-Specker theorem derives the full results by the means of commutative operators alone without exceptions when considering non-commutative operators. (Kochen & Specker 1967). The final statement of the Kochen-Specker theorem, then, is as follows: any hidden-variable interpretation, in order to be non-contradictory, must abandon one or more of these three principles (Held 2018):

> **Value-definiteness**: all observables in a given system have definite values at all times.
>
> **Non-contextuality**: all properties in a quantum system are possessed by the system independently of a measuring event.
>
> **Operator projection**: all physical quantities of a quantum mechanical system have a bijective relationship to their respective operators in Hilbert space.

The theorem, although notoriously complicated in its proof, holds true, and thus it is indeed required that any interpretation accommodates it by abandoning one or more of the aforementioned principles. The path taken by the de Broglie-Bohm interpretation can already be seen: it abandons non-contextuality. This is a quite straight-forward consequence from the fact that the theory is able to reproduce the empirical content of quantum mechanics whilst preserving value-definiteness in its ontology. It can then be seen that the measurement instance must necessarily be a part of the total system, and that it causes the unavoidable statistical dispersion. Recall from III.II.II. the formalization of measurement as a part of the total wave function of the system. Due to non-local interference, the measuring apparatus affects the channels that are eventually selected, thus, causally, participating in the realization of the observed results. However, the epistemic uncertainty involved in this process cannot be eliminated, as that would require a functional empirical theory that included non-contextuality as well – which is exactly what is being ruled out by the Kochen-Specker theorem.

An interesting parallel to the contextuality of the de Broglie-Bohm interpretation are the views presented by Bohr. Both seem to be in a certain agreement regarding the holistic nature of the combined system of the measurement apparatus and the measured quantities when considering actual observations. Recall from II.II.III.III. Bohr's statement: "…I advocated the application of the word "phenomenon" exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement." (Bohr 1949, 24.) For Bohr, there is *no way* of speaking about the independent system without instruments being used in conjunction. Thus, one must restrict their whole class of statements regarding quantum mechanics to specific phenomena in specific circumstances. Bohr states elsewhere:

> [I]t is equally important to understand that just this circumstance implies that no result of an experiment concerning a phenomenon which, in principle, lies outside the range of classical physics can be interpreted as giving information about independent properties of the objects, but is inherently connected with a definite situation in the description of which measuring instruments interacting with the objects also enter essentially. (Bohr 1958, 26.)

It is again needed to stress that by indistinguishability, Bohr does not mean a weaker statement regarding the empirical measurement with some leeway on the possibility of separation between the system and the apparatus on some other ontological level. Rather he means a total ban on statements that aim to bring forth a picture of how different components interact with each other to make the said measurement come about – this is Bohr's "contextuality". To give this attitude more structure, I'll sketch two contextual statements:

> (C1) All observations of quantum mechanics refer to the total system involving the system being measured *and* the measuring apparatus as well as their surroundings.
>
> (C2) All statements concerning how separate components of the total system interact causally to bring the observation about, violating the epistemic limit of the uncertainty principle, are prohibited.

While Bohr makes both statements (Bohr e.g. 1949, 46), the proponents of the de Broglie-Bohm interpretation accept (C1) but reject (C2). The grounds for this is that, although unable to circumvent the uncertainty principle, the interpretation is non-contradictory while preserving value definiteness.[50] Bohm and Hiley formulate their contextuality on the *participatory* nature of the event of observation, and only aim to fade out the notion of wave function collapse in their framework of channels of active

[50] It is worthwhile to clarify that the value-definiteness of the de Broglie-Bohm interpretation, or Bohmian mechanics, concerns particle *positions*, but not other operators such as momentum or energy. This is a consequence of the dynamics in the interpretation: positions are defined in real space $\mathbb{R}^3$ where other quantities operate in configuration space of dimensions $\mathbb{R}^{3N}$. The positions are not, in this ontology, a part of the wave function (as a separate field) but other quantities, such as the velocity field, are. Reasoning for this is the way of formalizing the action of the quantum potential, as stated by Bohm & Hiley (1993, 61):

> The fact that the wave function is in configuration space clearly prevents us from regarding the quantum field as one that carries energy and momentum […] which leads us to consider the interpretation of this field as active information. The multidimensional nature of this field need not then be so mysterious, since information can be organised into as many sets of dimension as may be needed.

information. In describing the quantum processes independently of observations, they do not object to the fact that all observations require the system-modifying interaction of the measuring apparatus – they only argue that there *exists* an independent description of the kind they are presenting. They write:

> It is clear then that we are not 'measuring' a state that has already been in existence. Rather the apparatus and the observed system have *participated* in each other, and in this process they have deeply affected each other. After the interaction is over we are left, as already pointed out in the previous section, with a situation in which the states of the two are correlated, in accordance with the channel that the particles have actually entered. (Bohm & Hiley 1993, 107.)

However, other kinds of criticisms have been directed towards the de Broglie-Bohm interpretation. Of these, the most important is the suspicion that it does not actually say anything novel about quantum mechanics. Heisenberg, one of the most prominent contemporary critics of the interpretation, writes:

> This objective "description", however, reveals itself as a kind of "ideological superstructure", which has little to do with immediate physical reality; for the "hidden parameters" of Bohm's interpretation are of such a kind that they can *never* occur in the description of real processes, if the quantum theory remains unchanged. - - The first consequence of this is that Bohm's interpretation cannot be refuted by experiment, and this is true of all the counter-proposals in the first group [Heisenberg's category of purely philosophical interpretations]. From the fundamentally "positivistic" (it would perhaps be better to say "purely physical") standpoint, we are thus concerned not with counter-proposals to the Copenhagen interpretation, but with its exact repetition in a different language. (Heisenberg 1955, 18.)

On the same track was Wolfgang Pauli. In correspondence with Bohm, he stated:

> I do not see any longer the possibility of any logical contradiction as long as your results agree completely with those of the usual wave mechanics and as long as no means is given to measure the values of your hidden parameters both in the measuring apparatus and in the observed system. As far as the whole matter stands now, your "extra wave-mechanical predictions" are still a check, which cannot be cashed. (Pauli 1951, ref. Meyenn 1996, 436.)

These criticism are instrumental in giving light to the *motivation* of any realistic interpretation. The overall argument in these objections does not concern any properties of a first-order physical theory, and it is, in fact, perfectly summarized by Bohm in his reply to Pauli's letter:

> Since you admit the logical consistency of my point of view, and since you cannot give any arguments showing that it is wrong, it seems to me

> that your desire to hold on to the usual interpretation can have only one justification; namely, the positivist principle of not postulating constructs that do not correspond to things that can not be observed. (Bohm 1951, ref. Meyenn 1996, 442.)

This discussion ties back into the Bohr-Einstein discussions of 20’s and 30’s. As told in II.II.III.III, Einstein was dissatisfied with the lack of ontology in the received view of quantum theory, calling it a “bloodless ghost” (Einstein 1949, 3). This dissatisfaction motivated Einstein, but his methods for seeking reconciliation were purely epistemic. However, as was already discussed, Einstein’s methods did not work, and the no-go theorems give strong reason to assume that any such means are contradictory with quantum theory altogether.

An important methodological clarification is in order before proceeding forward. I have made several choices made up to this point which greatly impact the scope of what is being discussed in the latter part of the thesis. The choices are as follows:

- It is recognized that there is only one theory of quantum phenomena, namely the quantum theory. This is because every other formulation of quantum mechanics (i.e. other than those based on standard wave or matrix mechanics) produce exactly the same empirical predictions.
- Interpretations of quantum theory, as discussed in this thesis, are subsidiary to the theoretical structure of quantum theory. If there were a contradictory experiment to quantum theory, as would be the case if, for example, uncertainty relations were circumvented in order to gain information about an electron’s history, then the theory that predicted the experiment would be contradictory with quantum theory. This theory, then, would not be an interpretation of quantum theory.
- While the possibility of an experiment that refuted quantum theory is not overruled, this thesis only discusses the situation where quantum theory is not refuted.

These choices are arguably uncontroversial. The tension between the quantum theory and novel experiments bringing about new information was recognized already in the EPR paper by remarks such as:

> While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. (Einstein, Podolsky & Rosen 1935, 780.)

It was also explicitly stated by Bohm in 1957 that the possibility of sub-atomic experiments would also bring about, possibly, a whole range of theories distinct from the quantum theory:

> More important, however, is the fact that in terms of the notion of a subquantum mechanical level, we are enabled to consider a whole range of

> qualitatively new kinds of theories, approaching the usual form of the quantum theory only as approximations that hold in limiting cases. (Bohm 1957, 85.)

Moving forward, the constraints of quantum theory lead into the discussion of the *nature* of physics in general, or, what that nature *should* be. The question is: is physics a purely positivist enterprise or something more in terms of describing nature? For the proponents of the de Broglie-Bohm interpretation, physical theories should without a doubt strive towards a description of real physical processes. Now, to properly honour the approach of many of the proponents of the de Broglie-Bohm interpretation (including Bohm[51]), this means maintaining research and hypotheses beyond quantum theory in order to sustain the possibility for progress. However, the interpretative content pertaining to quantum theory, in connection to physical realism, is also argued for. Bohm and Hiley write:

> It follows from [Bohr's and Heisenberg's stances] that quantum mechanics can say little or nothing about reality itself. In philosophical terminology, it does not give what can be called an *ontology* for a quantum system. (Bohm & Hiley 1993, 2—4.)

This statement is echoed by Bricmont (2016, 179):

> [I]f ordinary quantum mechanics is a theory, what is it a theory of? If it is a theory about results of measurements, then it is not a physical theory, which is supposed to deal with the world outside of our laboratories and to be checked by experiments, but not be limited to them. If it is a theory about the world outside of our laboratories, then what does it say? […] [T]he answer to that crucial question is often ambiguous or even, when made precise, false (because no quantum state ever collapses under the ordinary quantum evolution, or because of the no hidden variables theorems). So the point of the de Broglie–Bohm theory is simply to be the missing theory behind the quantum algorithm.

The proponents of the de Broglie-Bohm interpretation also argue that the reason for the wide acceptance of standard quantum mechanics lies simply on historical order – the Copenhagen interpretation came first. On this, Goldstein (2017, 15) writes:

> [I]t is only with a purely instrumental attitude towards scientific theories that Bohmian mechanics and standard quantum mechanics can possibly be regarded as different formulations of exactly the same theory. … Those impressed by the "not-a-distinct-theory" objection presumably give considerable weight to the fact that standard quantum mechanics came first. Supporters of Bohmian mechanics give more weight to its greater simplicity and clarity.

[51] The present hypothetical status of the proposed physical processes was also at no point contested by Bohm.

Of note is that Goldstein refers here to Bohmian mechanics which is often used interchangeably with the de Broglie-Bohm interpretation. However, it more properly refers to the mechanical formalisms in the interpretation (as opposed to those of standard quantum mechanics). Goldstein also mentions simplicity and clarity as strengths of Bohmian mechanics. Similar statements are made, for example, by Bohm and Hiley (1993, 58—60). Such arguments, for one side or the other, are not considered here. This is because they are not of interest in the attempt of finding non-subjective standards for interpretations. However, there are reasons to find these sorts of statements dubious.[52] In any case, Bohm and Hiley double down on the argument from historical order (1993, 4):

> Let us suppose however that the Solvay Congress [of 1927, where de Broglie first presented the concept of the pilot wave] had gone the other way and that de Broglie's ideas had eventually been adopted and developed. What then would have happened, if 25 years later some physicists had come along and had proposed the current interpretation (which is at present the conventional one)? Clearly by then there would be a large number of physicists trained in the de Broglie interpretation and these would have found it difficult to change. They would naturally have asked: "What do we concretely gain if we do change, if after all the results are all the same?"

This discussion can be now be summarized. First and foremost, we have the objection from empirical equivalence:

> (OEE) The de Broglie-Bohm interpretation has exactly the same empirical content with standard quantum mechanics, and no other property in it can be justified on the basis of observations. It is thus exactly the same theory with a surplus of speculation that cannot be vindicated.

To this, we have the counter-statements from ontology and history:

> (CO) Although the de Broglie-Bohm interpretation has the same empirical content as the standard interpretation, it is nevertheless to be preferred because it provides an account of real physical processes which the standard interpretation fails to do.
>
> (CH) The reason the de Broglie-Bohm interpretation is not preferred is historical: the standard interpretation simply came first. In an alternative world, where the de Broglie-Bohm interpretation came first, the scientific

[52] From a purely instrumental point of view, it is of course perfectly reasonable to prefer formulations of greater simplicity and/or elegance, if they function as more efficient tools for attaining the desired results. Examples for the application of different techniques are countless for anyone familiar with physics. However, this is obviously far from making an argument for the ontological priority of the objects posited in a given framework. It is to be noted that these kinds of arguments have, however, been made in other contexts than the de Broglie-Bohm interpretations as well, such as in string theory. For further discussion, see Dardashti, Dawid & Thébault (2019, e.g. 109).

> community would object to the Copenhagen interpretation in a similar way than they do now to the de Broglie-Bohm interpretation.

A certain tension can be seen to appear between the two counter-statements. For if one accepts (CH), this is to say that, in a remotely Kuhnian fashion, the grounds for accepting a theory in a scientific community is due to the associated historical and sociological context. However, (CO) states that there are reasons related to the interpretations themselves for why one should be preferred over the other. The supposed tension is easily enough dissolved by stating the counter-statement from ontology *and* history:

> (CO&H) The de Broglie-Bohm interpretation should be preferred because it describes real physical processes. It would in fact be preferred, if not for the unfortunate historical timing of it. The scientific community is led astray by their insistence on the received view, and thus fail to recognize a viable alternative.

This is most certainly a statement a proponent of the de Broglie-Bohm interpretation agrees with. What is now pressing is whether there exists any argumentative way to find a philosophical resolution to the dispute between (OEE) and (CO&H) – which is to say, whether there is something to say about this that is not a matter of taste. Historically, the discussion on this matter has been mostly restricted to passages of a few sentences of the kind presented above. This subject, however, touches the topic of non-empirical confirmation in physics, which has been recently discussed by Dawid (2019, 99—119) and Rovelli (2019, 120—124). In his response to Dawid's article leaning on Bayesianism in theory-confirmation, Rovelli writes:

> Theorists do not develop theories at random. They use powerful theoretical, non-empirical, motivations for creating, choosing, and developing theories. If these did not exist, the formidable historical success of theoretical physics would be incomprehensible. To evaluate theories, they routinely employ a vaste [sic] array of non-empirical arguments, increasing or decreasing their confidence in this or that theoretical idea, before the hard test of empirical confirmation[.] (Rovelli 2019, 122.)

Later, he writes on the difference between established and tentative theories:

> In their domains of validity, classical electrodynamics and Newtonian mechanics are considered even more reliable: We routinely entrust our lives to them. No sensible person would entrust her life to a prediction of string theory.
>
> The distinction is there and is clear. A philosophy of science blind to this distinction is a bad philosophy of science. It is so important that phrasing it in terms of a higher or lower Bayesian degree of belief obfuscates the

> point: In science we do have theories that are "confirmed" or "established," which means that they are extremely reliable in their domain. Then we have other theories that perhaps enjoy the confidence of some scientists but are considered tentative: We wouldn't entrust to them even our life savings. (Rovelli 2019, 122.)

What Rovelli stresses here is the distinction between scientific discovery and justification, of which the latter is routinely understood as confirmatory observations. In this framework, a theory is tentative if there does not yet exist a (set of) observation(s) to confirm it. Naturally, a tentative theory is not invalid, but there must be a (set of) prediction(s) it makes in order for it to be a viable contender. Now, the discussion of (OEE) vs. (CO&H) touches on this topic, but does not fall under it, for the predictions of the Copenhagen and the de Broglie-Bohm interpretations are equivalent. Moreover and at the risk of repetition, from the Kochen-Specker theorem we know that there is no possibility for the predictions to differ. What this means is that they are indeed stances on the *same theory* (which means talk of the de Broglie-Bohm interpretation as a theory is misguided).[53] This component of (OEE) is thus granted.[54] However, this does not solve the dispute, for even as we accept that the theory is the same, it says nothing on the matter of preference of interpretation. The question that remains from (OEE) is: *can* the non-observational relations and entities in the de Broglie-Bohm be vindicated?

Moving forward, any historical or sociological arguments regarding the acceptance of any interpretation will not be discussed in any other capacity than as a challenge for this philosophical project: if any robust philosophical reasoning for the priority of one interpretation over the other cannot be found, then I concede that a sociological reason is as good as any other. In order to avoid this rather uncomfortable situation and gain fruitful ground in this discussion, what is proposed in this thesis is a disassembly of the notion of *interpretation*.

In the following chapter, the use of this notion in the context of logic and scientific models is scrutinized in order to find some, if any, agreement on what the reference of the term is or should be. This is not done in order to fix a conclusive definition of the

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[53] This idea has already been presented by Quine in his "*Pursuit of Truth"* (1992, §41).
[54] In order to alleviate concern that logical positivism is slipped in through the back door here, it can be said that it is rather brought in from the front. Strictly, in the domain of physics, the only way to justify and sustainably establish empirical theories is observation. This says nothing of the process of discovery. More importantly, nothing is said here about the preference of interpretation – only the boundary conditions for established theories are outlined. I also want to stress that this statement is made only in reference to the field of physics; nothing is said of any other discipline. This also to say that the difficult and non-uniform relationship between discovery and justification across the sciences is duly noted – but here we are only concerned with theoretical physics.

notion, but to find some clarification to the discussion presented above.

IV. INTERPRETATION AND QUANTUM MECHANICS

IV.I. MODELS IN PHYSICS

To begin the theoretical treatment of the notion of 'interpretation' in quantum mechanics, it is natural to acknowledge an important discussion related to the notion. The field of model research in philosophy of science is vast, and contributions overlapping physics have been recently made for example by Suárez and Pero (2018), Suárez and Cartwright (2008), Contessa (2007, 2010), van Fraassen (2012), da Costa and French (2000, 2003), French (2010), and Mäki (2006, 2009). However, for the purposes of constructing a definition of interpretation in the relevant context, we will incidentally find the relevant pointers from the four articles discussed next.[55]

Frigg (2012) states that the most common use for the notion of model is a simplified and stylized version of the target system it represents.[56] The relationship between theory and models, in its simplest form, can be thought to be a transition from the general to the particular. By this train of thought, theories are sort of 'families' of models, while models themselves are applications of the theory in certain situations.[57] Fürth (1968) divides models in theoretical physics to into four categories:

F1 Functional models
F2 Structural models
F3 Scale models
F4 Analogue models

This categorization is useful for the purpose of this thesis, for it allows identifying and demarcating the kinds of models that are relevant in the context of quantum mechanics.[58] By (F1), functional models, Fürth means mathematical formulae meant to describe and predict the behavior of the target system. Naturally, the equations need to

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[55] The papers discussed here are fairly old – from 1953, 1968 and 1980. However, no discussions of models relevant to this topic and constrained to the domain of physics have been published recently. The content of these papers, in any case, is what matters – and as such, the relevant points for the purposes of this thesis are already made during the 20th century.

[56] No discussion regarding models as set-theoretical structures with isomorphic relations to their target systems will be entertained here. However, they will be touched in the following chapter in the context of model theory.

[57] However, the distinction between theories and models is not and need not be so straightforward, as theories range from more general to less, and they also always require interpretation upon application.

[58] This is not to say that the categorization is conclusive or even the most descriptive one made: just that for isolating the notion of "interpretation" in the context of QM, we have a good freeway here, as we shall shortly see.

be simplified in order to be generalizable – it is impossible for a model to share a 1:1 correspondence with the target system. Thus, these models can be thought as ‘idealized’ – but that does not mean they are ‘false’ in the sense that they could not provide reliable knowledge of nature. As a contrast, (F2) structural models are (a set of) assumptions regarding the aggregate behavior of an ensemble of systems. Examples would be a picturization of an object’s surface composed of atoms in a neat homogenous order, or the ‘electron cloud’ in the Rutherford model of the atom. Obviously, no neat order of atoms can be observed nor strictly inferred, but such a picture can be idealized from various scattering experiments. Similarly, the position of the electron in the nucleus’ ‘orbit’ can only be understood in statistical terms (in the context of the standard interpretation of quantum theory, at the very least), but it can also be visualized as a ‘cloud’.

As the (F3), we have scale models, which refers to usually macroscopic structures constructed in order to help understand some mechanisms behind mathematical formalisms. An example of this is a set of colored balls held together by pins as means of demonstrating atom bonding. Although these kinds of models usually share no strict consistency with the target system (in the atom bonding example, macroscopic objects are used to demonstrate microscopic mechanisms which obviously disobey macroscopic laws), they can still be useful in aiding understanding of the systems being studied, as long as the limits of their reference are understood. Finally, models can be analogies (F4), which Fürth takes roughly to mean that a mathematical description of some system can be usefully applied to some other system as well. A very common example of this is the use of harmonic oscillators to describe all kinds of systems exhibiting sinusoidal behavior, from springs and pendulums to the microscopic description of temperature.

Other takes on the kinds of models present in physics have been, of course, presented. The subject of models as analogues has been studied in greater detail by Mary Hesse. In her 1953 paper “*Models in Physics*”, Hesse differentiates between two uses of the notion:

a) An analogy between two branches of physics – this is the sense in which Fürth understands analogies. As a further example, Hesse provides the similarities between the theories of heat and electrostatics.
b) A general relation between a model and the target system – for example, the billiard ball model of gas molecules. Of this, Hesse writes:

> "When we say in this way that here is an analogy between a model and certain phenomena of nature, we are in some sense asserting an identity of mathematical structure between the model and nature, as in sense (a) we are asserting such an identity between two theories." (Hesse 1953, 201.)

To cut to the chase, in Hesse's framework, the sense (b) more or less encompasses Fürth's senses (F1) – (F3). A further take is provided by Redhead (1980), who identifies, at the very least, three *use cases* for models. Recognizing Hesse, Redhead admits that treating models as analogies "[…] hence may justify the physicist in pursuing the possibility that the model behavior is also exhibited by the full theory", but adds: "physicists should always be aware of the potential fallibility in this mode of reasoning" (Redhead 1980, 161). Redhead's warning stems from how he understands the notion of analogy:

> A *mathematical* model arises when we establish a formal analogy between a physical theory and some mathematical structure. This latter may often be embedded in some wider structure leading to the notion of 'surplus' structure in the mathematical formulation of a physical theory. The calculus associated with this wider structure is partially interpreted by means of the original theory *T*. This is typically how partially interpreted theories actually arise in physics. (Redhead 1980, 149.)

Redhead also identifies something he calls "floating models" – models that are neither inferred from underlying theory nor constructed from direct novel observations. This has to do with a so-called *computation gap*: if the target system becomes too complex to model directly by inferring from theory, and a model accounting for the empirical results is nonexistent, some approximation for a defined singular case can be used in order to aid further enquiry. This, of course, has a lot to do with heuristics. Redhead stresses that heuristic properties of models fall into the category of scientific discovery and, as such, are difficult if not impossible to systematize. This is echoed by Hesse, who writes: "The main point … is that there can be no set of rules given for the procedure of scientific discovery[.]" (Hesse 1953, 198.)

Here, we have a natural connection, again, to Fürth's senses (F2) and (F3). Both structural and scale models obviously have a strong heuristic function. Furthermore, in these sorts of cases, the simplest form of the relation between theory and models is inapplicable. And at the risk of stating the obvious, when dealing with models of heuristic priority, the scientist must take care to properly attend to the epistemic limitations of the given model.[59] In any case, a common thread of this discussion in theoretical physics is the admittance that, in some cases, no other model than a mathematical formulation can be found – and in these sorts of cases, the mathematical formalism itself has to be accepted as the model of the given phenomena.[60] Redhead's view on the matter is reflected in the quote above. Further, Hesse writes:

> Mathematical formalisms, when used as hypotheses in the description of physical phenomena, may function like the mechanical models of an earlier stage in physics, without having in themselves any mechanical or other physical interpretation. (Hesse 1953, 189—199.)
>
> In many cases the real progress was made in terms of a mathematical model: the mechanical model was then added only as an afterthought in the mistaken belief that it endowed the mathematics with a respectability it would not otherwise possess. (Hesse 1953, 212.)

Thea above is also a premise of Fürth's functional models:

> It is the generally accepted view that the laws of physics are expressed in the form of mathematical equations between certain variable quantities or 'parameters' which may either be capable of assuming any values within a certain range (continuous parameters) or are restricted to a finite or infinite, but enumerable set of discrete values (discontinuous parameters). (Fürth 1968, 327).

What we are of course interested in here, for the time being at least, are the formalisms of quantum mechanics and their relation to the discussion of models. It is the view adopted in this thesis that Fürth's category of functional models in fact describes the relation between the mathematics of quantum mechanics and the observations they refer to adequately. The reason for this selection will become more apparent later in this section. However, it is already apparent that any possible mathematical application of quantum theory is an as-accurate-as-can-be description of the system producing the observed phenomena. They serve no specific heuristic role outside of describing the system, nor are they structural or scale models. Shortly put, what one is dealing with when considering the formulae of quantum theory is the description of as small physical processes as can be described. Of functional models, Fürth writes further:

> In many cases the parameters referring to a functional model can directly be identified with the parameters of a real system. For example the quantities 'current intensity' and 'potential' appearing in Ohm's law, which

[59] The statement would be: heuristic models do not have any necessary connection to the actual phenomenon outside of helping to understand some features of it. Thus, overextending the reference of the model can and will lead to false conclusions about the object of the model.

[60] Another way of saying this is: in physics there are situations where no other kinds of models (as in, for example, actual pictures) of a system can be assigned than the mathematical description without losing accuracy.

> applies to a fictitious 'linear circuit' model, can be identified with the quantities measured by means of galvanometers and electrometers on real, three-dimensional electric systems. On the other hand, especially in the case of 'quantum' models, some of the quantities appearing in the mathematical relationships meant to represent the behavior of the model, like 'wave functions' and 'matrices', are only indirectly connected with 'observable' parameters. For instance the wave function representing a beam of electrons is only related to the relative frequencies with which the electrons within the beam proceed in various directions and which can be observed by suitable measuring devices, but not to the movement of the individual electrons. (Fürth 1968, 328.)

In quantum mechanics, it is *in this discussion* natural to think of the equations predicting the behavior of physical systems themselves composing the most accurate models; although there are many visualizations constructed in service of especially heuristics, they have inevitable limits in terms of description.[61] In a purely empirical science, the model that predicts the observations is the model to use. Along the same lines is Hesse, who writes:

> All that the physicist can certainly determine about the nature are experimental results, usually expressed by measurements, and therefore the assertion of an analogy must mean at least that there are resemblances between these results and the model. The resemblances are in fact correspondences between the observed measurements and certain numbers deduced from the model; for example, if the appropriate calculations on the theory of mechanics are made about the energy of colliding billiard balls, we can obtain a numerical value which is the same as that shown on the scale of a thermometer placed in a vessel containing a gas. (Hesse 1953, 201.)

To understand the nature of possible quantum-mechanical models one can think of, for example, the already familiar quantum tunneling case. For the Schrödinger equation in a one-dimensional space, we write:

$$-\frac{\hbar^2}{2m}\frac{d^2}{dx^2}\Psi(x) + V(x)\Psi(x) = E\Psi(x).$$

When $V - E \neq 0$ and constant, we get:

$$V - E < 0 \Rightarrow k^2 = -\frac{2m}{\hbar^2}(V - E) \quad and \quad V - E > 0 \Rightarrow q^2 = \frac{2m}{\hbar^2}(V - E)$$

[61] However, properly interpreted they do have a lot of value in very specific circumstances – such as the graphical tools used by engineers in the study of quantum-mechanical systems. But as a *general* description of quantum physics there is reason to take their limit of adequacy into account. (As a natural example, a visual representation of a wave packet does not justify the scientist to think of the system of actually consisting of such wave packets.)

where the terms *k* and *q* are of course derived from the wave function. For this case, what we see above *is* the (general and idealized) model of the probability amplitudes in quantum tunneling, from which the respective probabilities are derived. Some understandable questions might arise: the model makes use of entities, such as the wave function in superposition of a range of states, which do not map directly to any observables[62] – how can they be part of any model? A straightforward answer would be to appeal to the purely instrumental nature of the functional model – it exists to formalize the observations made in a unified framework. The wave function is a *root object* in the model from which the observations, by appropriate operations, can be predicted (up to the ultimate limit of accuracy). To make this clearer, there is an epistemic reference for the wave function: it represents the probability amplitude from which expectation values etc. are derived in correspondence to measurements. A functional model does not need to ontologically correspond to the observations made, in the direct sense, as long as, again, the epistemic limits of the model are acknowledged. Fürth seems to agree with this:

> On the other hand most scientists are convinced that the fundamental laws of nature are essentially simple, and the search for these laws is characterised by the attempts to generalise, to simplify, and to unify established relationships. The question arises how these apparent contradictions can be resolved. The answer is, I believe, that in actual fact the parameters, appearing in the equations meant to express the laws of physics, do not refer to actual physical systems but to fictitious systems which are constructions of the theoretical physicist and only bear a certain resemblance in their behaviour to real physical systems. (Fürth 1968, 328.)

In this case, this *certain resemblance* is simply that the observations made in quantum physics correspond to the predictions from the equations of quantum mechanics. Here, the relationship between model and theory is also straightforward: quantum mechanics encompasses the equations of subatomic systems and the understanding of their reference to observations, and these equations are then applied to particular cases. Thus, the simple form of the relationship, as introduced by Frigg (2012) above (i.e. that models are instances of theory), is indeed the case here.[63]

[62] A challenge to this view is briefly discussed later in IV.IV.
[63] When confirming this relationship, only the mathematical formalism of standard quantum mechanics is considered, without any further explanation by the means of natural language, visualizations and so forth. The reference of the terms in the formalism to the physical system is, then, the matter of interpretation.

For transparency, I want to re-enforce the stance that only empirical observations can justify a theory of physics. The attitude is also shared, for example, by Hesse, who writes about the early equation of conduction of heat by Fourier:

> All that is necessary is to lay down the principle that the rate of flow of heat between two surfaces of the body is proportional to their difference of temperature[.] […] The only further assumption involved is that the flow and the temperature are continuous within the body where measurements cannot be made[.] […] [I]t is not necessary to enquire further into its nature; nothing would be added to the mathematical description in doing so. (Hesse 1953, 204—205.)

To be clear, this is enforced in parallel to the view adopted in this thesis that models in quantum mechanics, which we are only concerned with, are indeed particular instances of theory.[64]

Now, we are rapidly approaching the topic of interpretation, of which there is little direct literature in the context of quantum mechanics – the literature for "interpretations of quantum mechanics" is of course massive, but little attention has been paid to the concept itself. However, there is the clear fact that without some sort of interpretation, the mathematics of any physical theory would have no reference to the nature it is meant to predict and describe. This acknowledgement is, of course, shared by all. For example, Hesse writes: "All that can […] be said with certainty is that there is a similarity of mathematical structure between the model and the experiments[.]" (Hesse 1953, 203.) The notion has apparent overlap with other ways of phrasing, such as "relationship", "representation", "analogy" and so forth, but what is meant at all times is the way we understand *what the theory or the model is about*. This reference is primary for the understanding of the concept of interpretation in any context. Whether this reference is true, false or something else is secondary.

This is not a simple question. However, clarity can be found in the context of quantum mechanics by paying close attention to it. Some statements made by Hesse, Fürth, Redhead and Frigg will be returned to shortly. But first, let us inspect the notion of 'interpretation' in general. The angle of approach in what follows will be: how has the concept been understood in philosophical literature? Is there a connection between a philosophical understanding of the notion and the interpretations of quantum mechanics? If there is, what kind of a connection is it? If not, how could *interpretation*

[64] A qualifier must be left here: this is not to say that all possible models, existing or upcoming, in quantum physics, necessarily fulfil this relation. I only mean that the equations of quantum mechanics, which we are concerned with in this thesis, do.

of quantum mechanics be understood?

IV.II. INTERPRETATION

IV.II.I. SEMANTICS AND INTERPRETATION

The literature broaching the subject of interpretations can be roughly divided into semantic logic and, for the lack of a better word, the discussion of semantics of description[65]. I will comb through both – and then circle back to the discussion of interpretations of models. The object here is not to search for a proper definition of truth, but to give a best effort of a uniform description of the use of the notion of interpretation. In logic, it is defined as a property of a semantic system. In general, interpretations are related to truth conditions of sentences.

Alfred Tarski's work in semantics is, arguably, the widest known treatment of truth conditions of sentences. It is the foundation of most research in truth in modern logic as well. The Tarskian treatment of truth conditions – the T-model – first introduced in 1933, can be summed up in the following way (Tarski 1936, 275—276):

> (1) A primitive statement "A" is true if, and only if, A.
> (2) "¬A" is true if, and only if, "A" is not true.
> (3) "A∧B" is true if, and only if, "A" is true and "B" is true.
> (4) "A∨B" is true if, and only if, "A" is true or "B" is true or ("A" is true and "B" is true).
> (5) "∀x(Fx)" is true if, and only if, for every object x "Fx" is true.
> (6) "∃x(Fx)" is true if, and only if, there is an object x for which "Fx" is true.

To sum up these statements, they form a set of by which expressions can be ruled to be true or false. They form the axioms to which any given evaluation of truth in a sentence can be reduced to at the most primitive level. Of note is that for Tarski the function of the T-model is not to give a theory of meaning, but rather it gives the logical conditions for the truth values of expressions.

As an important clarification, this is a logical theorem of truth-conditions in language by the means of variables and logical connectives. However, for Tarski an important goal was to also formulate a model-theoretic approach for evaluating the truth-conditions of models in formal sciences. By *formal*, we mean deductive sciences (i.e. mathematics). Model theory, along the lines of Hodges (2013) means roughly the

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[65] This is to be sharply differentiated from descriptive semantics, which, for Carnap (1948, 11) means the "empirical investigation of the semantical features of historically given languages". The nature of this discussion has to do with the application of semantics to empirical models, which will be shown shortly.

following: take an arbitrary sentence *S* without any information of its meaning. At this stage, the sentence *S* cannot be deemed to be true or false, because an interpretation *I* of *S* is missing. Upon adding the interpretation *I*, if it makes *S* true, *I* becomes a model of *S*. Equivalent is the statement that *S* becomes true in *I*. Thus, *an interpretation of S is a model of S, if S is true in the interpretation*. From this, we get a class of interpretations that are models of *S* denoted Mod(*S*). Extending this to a set of sentences *T* we get Mod(*T*) – and here we can regard *T* as a set of axioms or a *mathematical theory*. Now, *T* and Mod(*T*) are to be understood as set-theoretical entities in order to explicate their relation to one another.

We now have grounds for a model-theoretic definition of truth. Following[66] Tarski & Vaught (1956): take two *relational systems*, here *T*=(*B, S*) and Mod(*T*)=(*A, R*), where *A* is a set of objects in *T* and *R* is the set of their relations. We also have arbitrary sequences $x_n \in A$ and a formula ϕ. An interpretation function $f(I)$ maps Mod(*T*) isomorphically to *T*. Then, a sequence x_n satisfies ϕ in Mod(*T*) if and only if $f(I(x_n))$ satisfies ϕ in *T*. Now, suppose we have a 'sentence' *S* belonging to *T*. By the isomorphic relation, *S* becomes true in Mod(*T*) if every sequence $x_n \in A$ satisfies *S* in Mod(*T*) (i.e. Mod(*T*) is a model of *S*). All of this is to say: we have semantic interpretations in the model system for the elements and relations of the object system – and these interpretations are understood through truth conditions.[67]

The undertaking of extending model theory from formal sciences to non-formal, that is, empirical, will be discussed shortly. In any case, building upon Tarski's 1933 treatment of truth conditions, Donald Davidson presents his account of *truth-conditional semantics*. It injects an account of *meaning* into Tarskian truth conditions. Davidson's motivation was to give a description of *how* meaning and expressions are related. In his article "*Truth and Meaning"* (1967) he writes:

> While there is agreement that it is the central task of semantics to give the semantic interpretation (the meaning) of every sentence in the language, nowhere in the linguistic literature will one find, so far as I know, a straightforward account of how a theory performs this task, or how to tell when it has been accomplished. (Davidson 1967, 308.)

[66] Here, the definition is cut down and notations changed to be in line with previous text. This is done in order to get the idea of the notion of interpretation in this context; further application of this definition is not a goal of this thesis.
[67] For further discussion on model theory, see e.g. Hodges 2013, and for Tarski's truth theory in general, Hodges 2018.

Davidson proceeds in showing how previous circular attempts to explicate meaning can be circumvented. The earlier issue encountered in a Fregean tradition was, in short, that the meaning of a sentence was linked back to meanings of the sentence components, the meanings of which could not be cashed out in explanatory terms. The solution Davidson suggests is to map meanings to truth values of sentences, thus analysing them without vagueness. Davidson writes (Davidson 1967, 309):

> As a final bold step, let us try treating the position occupied by ‘p’ extensionally: to implement this, sweep away the obscure 'means that', provide the sentence that replaces ‘p’ with a proper sentential connective, and supply the description that replaces ‘s’ with its own predicate. The plausible result is
>
> (T) *s* is *T* if and only if *p*.

Where *s* is the sentence, *T* is the interpretation of the sentence, and *p* is the set of truth conditions for the sentence. In other words, if the proposition “snow is white” is true if and only if snow is white, then the state of snow being white is the meaning of the aforementioned sentence. Here interpretation is equated with meaning – which is provided by Tarski’s theory of truth conditions.

Closely related to Davidson’s Tarskian treatment, Rudolf Carnap in his *“Introduction to Semantics”* (1948) takes the set of interpretations in a semantic system as rules for statements within that system, which define whether a given statement is true. For instance, using Carnap’s example, among the conditions for a statement *Pa* containing the proposition “the Moon is spherical” is the actual fact that the Moon is spherical. In order for this to work, is to be noted here that to assert that a sentence is true is the same as asserting just the sentence. In a semantic system, the statement *Pa designates* this proposition, thus “the Moon is spherical” is an interpretation of it. Carnap writes (1948, 22):

> By a semantical system (or interpreted system) we understand a system of rules, formulated in a metalanguage and referring to an object language, of such a kind that the rules determine a truth-condition for every sentence of the object language, i.e. a sufficient and necessary condition for its truth. In this way the sentences are *interpreted* by the rules, i.e. made understandable, because to understand a sentence, to know what is asserted by it, is the same as to know under what conditions it would be true.

In *“Introduction to Symbolic Logic and Its Applications”* (1957), Carnap doubles down on the use of interpretation as the relation by which we understand any statement. This is done in the context of Carnap’s introduction of the “language B”, which, briefly characterized, is a language containing connective signs, special signs, sentential constants, individual signs, predicates and functions. The language B, essentially, differs from the previously introduced language A in that where A is a purely syntactical system, B is additionally semantical.[68] Thus, the semantical system must include an interpretation of the language, which is analysed in the customary truth-conditional way. Taking an arbitrary sentence *S* belonging to B, an interpretation of it is connecting *S* to a truth-condition *p*, such that *S* is true if and only if *p*. Here, however, Carnap stresses also the process of creation of the interpretation – interpreting a system is not something done post-hoc, but which is often inseparable from the creation of the syntactical system. Carnap writes (1957, 101):

> One who constructs a syntactical system usually has in mind from the outset some interpretation of this system. (This interpretation need not itself have a prior representation as a semantical system; and indeed, what prior representation it may have is normally non-systematic.) While this intended interpretation can receive no explicit indication in the syntactical rules – since these rules must be strictly formal – the author's intention respecting interpretation naturally affects his choice of the formation and transformation rules of the syntactical system.

To be clear, what is in question here is the interpretation of Carnap's theory of syntactical systems – this has no necessary bearing on the statements of physics.

As the last example of authors writing on interpretations, we have Michael Dummett. Criticizing the truth-conditional theory, Michael Dummett argued that the meaning of expressions should be grounded on *proof* rather than *truth*. This is a bridge for *proof-theoretic semantics*. Where Tarski and Carnap represent a classical notion of meaning – which separates 'truth' and 'knowing the truth'. For Dummett, there is no distinction. Along the lines of a Fregean notion of reference, Dummett (1996, 234) writes:

> A semantic theory is not itself a theory of meaning, since it does not concern itself with what is known by a speaker and constitutes his grasp of the use of an expression: a knowledge of the meaning of a predicate does not consist in knowing of which objects it is true and of which it is false, and a knowledge of the meaning of a sentence does not consist in knowing its truth-value. But a semantic theory is plausible only in so far as it provides a base on which a theory of meaning can be constructed. The semantic theory seeks to exhibit the manner in which a sentence is determined as true, when it is true, in accordance with its composition, its

[68] The structure of "Introduction to Symbolic Logic" is two-fold: the first part defines an extended syntactic language and the second its application. The first part is further divided into three subsections: languages A, B and C. The language A is a simple syntactic language much in the tradition of classical symbolic logic. The language B introduces a semantical system on top of the base system. The extended language C also contains all expressions that can be made in A and B. Part two is the application of logical languages in non-formal context.

> internal structure. It does so by specifying, for each type of expression, what has to be associated with an expression of that type in order that, for every true sentence in which the expression occurs, we can exhibit the manner in which that sentence is determined as true in accordance with its composition.

Here, whether in formal or natural language, the notion of interpretation means roughly the following: an operation of understanding the references of objects in the given language. This is stated by the theory of meaning, which is built upon the base semantic theory. Similar to Davidson, however, interpretation is still the exact same notion as meaning.

Before moving further, let us do a quick round-up. We seem to have two main lines of understanding of what, roughly, an interpretation is in the context of language and logic:

1. The relation which maps truth-conditions to statements in a language (Davidson, Carnap and Dummett[69])
2. A structure in a metalanguage which is a model of a structure in an object language insofar as it makes the structure true (Tarski and model theory)

Here, a strictly analogous attitude to sense (1) in fact makes some sense in connection to quantum theory. Although one must be careful when postulating connections between syntactical systems and scientific theories, a heuristic way to think about interpreting quantum theory shares some of the DNA with sense (1). One can think of a theory containing variables and relations, and hold that whether these are correct is decided on whether the theory actually predicts the phenomena connected with microscopical systems (i.e. these observable phenomena become, strictly analogously, the "truth-conditions" of the theory).

Thus, we already understand what the theory refers to in virtue of being able to use it in connection to empirical observations, and this understanding is what allows us to deem the theory as either "true" or "false". This, albeit quite trivially, is the interpretation of the theory. Here, along the lines of the above Carnap quote, the interpretation is, of course, simultaneously known when the theory is created. However, this analogy is as far as one can go with this, because, for example, quantum theory and any models it contains are not reducible to some statements held together by some syntax – to argue

[69] Although Dummett distances himself from the classical treatment of meaning, the base semantic system still, naturally, rests on truth-conditions – thus, I will stack Dummett in this same tradition.

something of this ilk would be to raise the ghost of the deductive-nomological model, which is preferably left to rest in peace.[70]

To actually get closer to the discussion of models, we need to turn to (2). Now, in the form presented here, this sense needs some special attention before moving forward. Quoting Frigg (2012), what the model-theoretic treatment means is: "If all sentences of a theory are true when its symbols are interpreted as referring to either objects, relations, or functions of a structure *S*, then *S* is a model of this theory." Frigg continues to build a bridge from this to physics: "Many models in science carry over from logic the idea of being the interpretation of an abstract calculus. This is particularly pertinent in physics[…]"

What the model-theoretic approach means to say is that a model is an interpretation of a theory that makes the theory true. In the model-theoretic framework, these models, as particular applications of the theory, are *interpretations* of it. The problem that one then faces is that there are said to be different interpretations of quantum theory. The consequence of this is that interpretations are different *models* of the theory, mapping the theory to different truth-conditions. What makes this situation peculiar, then, is the fact that a truth-condition that differs from the predictions of quantum mechanics must be non-empirical (assuming no experimental results that violate quantum theory are found).

However, a scaffolding for interpretations can be found. First, for the sake of the argument, we accept the model-theoretic sense of interpretation. We can then regard quantum theory as an abstract collection of formulas, without reference to empirical observations as such. We further take models as the applications of these formulas to particular situations with their respective empirical observations (i.e. the "truth-conditions"). Then the models can indeed be thought of as interpretations of theory. Importantly, however, this means that the model is, at the very same instant, two things:

1. A particular application of the theory, and
2. an understanding of how the theory refers to observations

This line of reasoning is somewhat satisfactory. However, we know for a fact of the formation of quantum theory that it was constructed to describe observed phenomena – thus it may seem odd to require a separate notion of interpretation, or model, to connect

[70] The DN model follows from the idea that particular cases can be explained by the base theory by means of inferences. The inadequacy of this has been presented in numerous occasions. (See e.g. Woodward, 2003.)

it post hoc to the very thing it was already describing. Be that as it may, in the way proposed above, interpretation, once again, becomes a notion of understanding the reference of theory to phenomena.[71]

A further development was in fact attempted in order to bridge the gap between formal and non-formal sciences – this is to say, there was an attempt to construct a syntactical system for non-formal sciences which would unify the language used in them. The draw of this, for the purposes of the inquiry at hand, is clear – if there were a syntactical system encompassing physics, we should not have any difficulties in deciding what the interpretations of theories in physics are. Next, I will briefly go over this attempt.

IV.II.II. MODEL THEORY AND DESCRIPTION

Carnap, in *"Introduction to Semantics"* (1948), introduces a distinction between *logical* and *descriptive* signs. As logical, we of course take connectives, operators and such. As descriptive, we take classified names of items, signs descripting empirical properties, empirical functions and so forth. This is done in order to build a bridge from strictly logical languages to languages describing the empirical world. But a more ambitious project is, already, on the horizon for Carnap – extending descriptive expressions to a system of *general semantics*:

> So far we have discussed the distinction between logical and descriptive expressions only in the form in which it appears when we have to do with a particular semantical system, in other words, as a question of special semantics. The problem is more difficult in the form it takes in *general semantics*. Here it is the question whether and how 'logical' and 'descriptive' can be defined on the basis of other semantical terms, e.g. 'designation' and 'true', so that the application of the general definition to any particular system will lead to a result which is in accordance with the intended distinction. A satisfactory solution is not yet known. (Carnap 1948, 59.)

Carnap gives a shot at such a language in his later *"Introduction to Symbolic Logic and its Applications"* (1957). The first part of the book introduces three languages, A, B and C, and the second part their application to descriptive situations. In order to do this,

[71] It is again of importance to emphasize that no theory of "how models refer to the world" is attempted in any way in this thesis, although some pointers are inevitably found. I will also emphasize that theories of physics have a relationship to measurements without any need to discuss reality itself further. Moreover, the main goal of the undergoing treatment is to isolate a proper definition, or at the very least some boundaries, for the notion of 'interpretation'. Some commitments have to be made in order to do this properly; the most significant is the commitment to the view that in quantum mechanics models are mathematical formulations applied to particular use cases.

Carnap introduces “the axiomatic method”, which is derived from the standard idea of axioms being self-evident truths and theorems being derived from axioms. However, Carnap states that in an axiomatic system (AS) of scientific language, arbitrary sentences may be selected as axioms. An AS is formulated in conjunction with a basic language of the AS which Carnap calls “language L”. When an axiomatic system is stated, it is assumed that the interpretation of its language L is understood. Carnap writes:

> When an interpretation of the primitives is given, the remaining axiomatic constants straightway receive an interpretation through their definitions[.] (Carnap 1957, 172.)

Here, we get the distinction between logical and descriptive. If all the primitives in an axiomatic system are interpreted as logical constants, we have a *logical interpretation* of the AS. Conversely, if we have any other interpretation belonging to the AS, we have a *descriptive* interpretation. The notion of “model” is analysed in the same vein as before in the model-theoretic treatment. However, Carnap recognizes that the situation changes when we are dealing with models of, for instance, empirical theory:

> By a *model* (more specifically, a logical or mathematical model) for the axiomatic primitive constants of a given AS with respect to a given domain *D* of individuals we mean a value assignment *VA* to these primitives such that both *D* and *VA* are specified without the use of descriptive constants. A model is said to be a *model of the AS* provided it satisfies all the axioms of the AS. […] The study of models is simpler than that of interpretations, since it deals with extensions, not intensions; e.g. with classes, not properties. Logical interpretations are essentially the same as models. […] However, if we are interested in the use of a given AS in fields of empirical science, e.g. physics, economics, etc., or in the construction of an AS as a formal representation of a given scientific theory, then we have to consider descriptive interpretations. (Carnap 1957, 173.)

The extension of models to empirical sciences was later on picked up especially by Apostel in his 1961 his paper *“Towards the Formal Study of Models in the Non-Formal Sciences*”. Here, Apostel acknowledges Carnap’s distinction of logical and descriptive interpretations, and uses this as the basis of studying the possibility of a formal theory of empirical models. The division cuts at the level of variables in a given syntax, or, in Carnap’s terminology, calculus. Here, the variables in the calculus are logical, if their range of values is defined strictly in the logical system. Contrastingly, they are descriptive, if their range is defined in a descriptive expression in the metalanguage. This is a natural entryway for the notion of interpretation in the context of scientific models, as it can be treated as the descriptive relation from the metalanguage to the model. A summarization of the difference between the two is given by Apostel (1961, 132):

> An interpretation is a true interpretation if whenever a sentence implies another in the calculus, in the interpretation whenever the first sentence is true, the second is equally true, and whenever a sentence is refutable in the calculus, it is false in the model. Such a true interpretation is a logically true interpretation, if the sentences that become true, become logically true. An interpretation is a factual interpretation if it is not a logical interpretation. An interpretation is a descriptive interpretation if at least one of the undefined signs of the calculus becomes in the interpretation a descriptive sign[.]

Apostel wants to outline a unification of all kinds of models in some formal way. Of course, we are not interested in this goal here as such, but the procedure of constructing a formal framework for empirical models, necessarily, involves some formal notion of their interpretation as well. Additionally, in this framework, there has to be some sort of treatment of the phenomena in quantum mechanics. Moving forward with the framework of empirical models, Apostel recognizes the problem of overdetermination with the idea of models being any interpretations of a theory that made the theory true. A natural example of this is the very existence of non-standard models in science (such as those in quantum mechanics). What becomes then relevant is the selection of not just any model, but an *intended* model of the theory. There is an important connection here to our problem of two different interpretations of quantum theory. We will return to this shortly.

Recall that we are using the notion of model in the model-theoretic sense as discussed above. Here, the basis for the definition of model is of the following kind: taking again a theory T, we define for it a model M, with the conditions that there exists a structure N, which is a set of observations with a certain relation to M, and is generally homomorphic to the class of all models K on T. This does not solve the problem of the intended model but it has an important characteristic – in formal model theory, the M of T is the same as the set of conditions which make T true (i.e. the "true-making interpretation", as it were). Here, M is examined in reference to N, which is a set of observations. Moreover, to avoid irrelevant discussion of scientific realism in this context, we use notions such as, to quote Apostel, "propositions of the formalism are verified[72]" with regards to the observations. The reason we need the set N is because no

[72] The notion "verified", of course, does not imply a commitment to verificationism, but just that our theory "checks out" as we observe the predictions it makes.

empirical theory can be said to completely describe the world, or, completely describe (in whatever sense of the word) the domain it refers to. If this were the case, we could just say that the world is the model of the theory. This is considered by Apostel (1961, 133):

> [I]t is clear that the concept of model in the empirical sciences, when it is used in the following context 'the world is a model of our sciences, in as far as these sciences are true' (or conversely the aim of science is to construct a calculus for which reality is the only model) takes the concept 'model' in the sense of a factual and descriptive true interpretation.

We will not venture further into the possibility of a formal theory of empirical models (regarding quantum theory or any other theory), but some important notes are taken on board. Apostel's idea of models as the intermediary between theory and the world is in line with the view of quantum-mechanical models as particular instances of theory, as accepted in the context of Fürth's functional models.[73] Now, we can start building a picture:

1. There is the quantum theory, which, while of course not in any practical way disconnected from the world it is built to describe, can be thought of an abstract formalism.
2. This formalism is then to be measured against the world in *the way we interpret* its reference.
3. In order to do this, we apply the theory to particular instances where observations are being made. Thus, we get a model, from the theory, to the particular case, and this particular model, then, predicts the kinds of results we will obtain. Here, we have *interpreted* the theory for this situation.
4. Upon obtaining the predicted results, we see that the model checks out, and, as the model is a direct application of theory, the theory is verified (once again).
5. As an obvious consequence, we see that we have both *interpreted* the theory, and interpreted it in the way that verifies it.

However, from this is still omitted the fact that, once we get the model for the particular situation, we also interpret the model in connection to observation. This point might not be of crucial significance, because the complete interpretation of theory in connection to any particular observational situation takes place at once when the theory is applied. In any case, let us turn to models in physics for the last time.

[73] By this statement, of course, no such argument is made that would defend the idea of model theory as proper means of formalizing models of quantum mechanics – or any semantical or syntactical system for that matter.

IV.II.III. MODELS AND INTERPRETATION – ROUND TWO

As a convenient summary of the discussion referenced in the preceding sections, Apostel sketches the relationships between theories, models and interpretations for empirical sciences (Apostel 1961, 127):

> Models are then introduced to constitute the bridge between the theoretical and observational levels, the theoretical predicates being interpretable as predicates of the model and the observational predicates being also interpretable as predicates of the model, the model furnishing lawful relationships between the two interpretations.

Here, we can equate "observational level" to the physical phenomena that models are taken to represent. This can be put in other words: as the scientist is crafting a theory that unifies the physical phenomena she has observed, she creates a model containing the objects and relations she interprets the phenomena to have. The interpretation of these objects and relations in the 'model reality' coincides with the interpretation of objects and relations of the empirical world. This process is, of course, not arbitrary, but governed by what she can observe and distinguish in the empirical world. Fürth (1966, 329) expands on this relation:

> When a definite connection has been established between the parameters describing a functional model, and those of the real physical system it is meant to represent, the next step must be to try to 'verify' by means of measurements on the real system, whether the mathematical law for the model describes the behaviour of the real system correctly; only if the mathematical relationship is confirmed by the measurements without exception can it be considered to constitute a physical law.

The scientist, of course, has only limited means for establishing these relations – for example, any physical measurement has a limited degree of accuracy which directly influences both model-creation and what can be said to be known from the model. Fürth writes further:

> Thus any statement about verification must be accompanied by a statement on the degree of accuracy of the type of measurements which were used for the purpose of verification. If the equations for the model correctly describe the relationships between the various parameters of the real system *within the stated degree of accuracy* then the equations are verified, and the model may be regarded as an appropriate representation of a physical reality on a certain level of observational technique. (Fürth 1966, 329).

What, then, happens when a model is able to correspond to all the physical phenomena it is meant to but also exhibits other kinds of objects or structures which are not observational? Fürth argues that these represent "more parameters than necessary":

> [O]ne of the fundamental principles of quantum physics maintains that there exists a natural and ultimate limit for the accuracy with which any observation can be carried out. If this tenet is accepted as true, the just mentioned process [referring to advances in measurement accuracy] can *not* be continued beyond this limit, and as the 'uncertainty principle' is actually built into the basic equations of quantum mechanics, no explicit statement concerning the degree of accuracy of the measurements need be made for the purpose of the verification of the mathematical relationship that apply to a functional model constructed on quantum mechanical principles.
>
> From this point of view it would appear to be superfluous to invent models containing a greater number of parameters than strictly necessary for the complete description of a physical system with the ultimate limit of accuracy demanded by the quantum mechanical principles … [I]f the uncertainty principle is supposed to be correct then the very notion of 'real physical laws', underlying the laws that are verifiable by observations with ultimate degree of accuracy, become meaningless. (Fürth 1966, 330.)

The notion of "meaningless" aside, as well as the apparent circularity in arguing for a natural limit of accuracy[74], here we actually have a proposition of a solution to the problem of the intended model, as discussed previously. Fürth's tactic would be to appeal to the *parameters* in the model and their relation to our capabilities to measure them. Any parameters that are not necessary to connect the model to actual results become superfluous (there is an obvious connection to Heisenberg's phrasing here!) – and such should be left out from the intended model.

Even if this might bring to mind a kind of hand-wavy positivism seen in chapter III, here we have in fact, with some adjustments[75], some kind of a philosophical argument as opposed to a simple dismissal. An intended model is such that it connects the theory to phenomena, and we need some kind of a criterion to pick the intended model from the class of all models verifying the theory. Thus the model which has only the necessary parameters for correct predictions is, at the very least, closer to the intended model. If we take models in the model-theoretic sense, there is an extension to interpretations here as well.

[74] To state with conviction that the uncertainty principle is physically fundamental, in some way or another, is as unwarranted by quantum theory than anything else. Bohm writes (1957, 64): "Thus, the conclusion that there is no deeper level of causally determined motion is just a piece of circular reasoning, since it will follow only if we assume beforehand that no such level exists." He is right.

[75] See the previous footnote. This kind of an argument cannot base itself in the fundamentality of the uncertainty principle if it is understood in terms of physical reality rather than quantum theory. However, if we refer to the theoretical structure of *quantum theory* itself (connected to the predictions it is able to ultimately produce), we can cash out the preferences for the model.

Something similar is taken up by Hesse in her 1954 paper. In her concluding remarks, Hesse states (Hesse 1954, 214):

> Not only has each model an indefinite number of pointers, but since there is an indefinite number of ways of adding to the mathematical structure which forms the basis of the analogy, there is also an indefinite number of different models of any given physical situation, each having the same set of formal rules, but having different pointers, some of which may contradict the others. … Short of some metaphysical postulate of the unity of nature there is no a priori reason why light should behave in the least like particles or waves, or why the fundamental particles (even the name indicates how far analogy permeates our thinking) should behave like gravitating planets or electrified pith-balls, or indeed in any way that can be described by existing mathematical theories.

Neither Hesse nor Fürth ascribe explicitly to any semantical or model-theoretic view of formal models. Moreover, Hesse does not here give any proposition of an intended model, explicitly or otherwise. But she warns about misconstruing a *physical situation* from a *model situation*, due to previous experience and intuitions leading the model-builder, or interpreter, astray. This brings somewhat to mind Heisenberg’s remarks quoted in chapter II. Furthermore, it is, once more and finally, echoed by Fürth in the discussion of scale models (Fürth 1966, 337):

> “The fact that it became necessary to ascribe both particle and wave properties to field and matter is usually interpreted as expressing and inherent duality in nature. In my opinion this interpretation is not correct. As already emphasized, the notions of particles and waves are taken from macroscopic experience. Both notions may be used for constructing scale models of physical systems in atomic dimensions in order to visualize physical processes in these dimensions. But one must not be surprised to find that they eventually become incompatible when one reaches sub-atomic dimension, and that one is forced to use one or the other according to the observational situation if one insists in using scale models even then.”

This is more or less equivalent to the discussion in chapter II about wave and particle pictures and their respective limitations. Of note here is that in this quote Fürth is using the word interpretation in the sense it is used in the context of “interpretations of quantum mechanics”, not in the more technical sense we are striving towards here.

In any case, a crucial understanding of interpretations is demonstrated in Mary Hesse’s later treatment of model analogies. In her 1970 paper *“Models and Analogies in Science”* she defends the notion of *formal analogies*. Shortly put, formal analogies are two or more different cases of reality that can be described by the same model. For example, trigonometric functions can model a number of different physical phenomena involving swinging, rotating or orbital motion as well as continuous alternating acceleration of any quantity. Thus, two cases are formal analogies if they can be observed to comply with the same model. Hesse writes (1970, 99):

> [I]f we regard a valid argument by analogy from models as essentially a transfer of causal relations between some characters from one side of the analogy relation to the other, it follows that the interpretation of theoretical terms we have just given is *presupposed* in the argument, even if not explicitly referred to, for if there is a theory about the causal relations in model (2), then the same theory holds for the relevant characters in model (1), and hence for the explanatory theory being sought.

The insight of *presupposition* is, again, important for the subject matter of quantum theory as well. Whenever a scientist is creating a model, whether the model is understood in the model-theoretic sense or otherwise, its interpretation is presupposed by her understanding of the observables she is modelling. Thus, interpretation isn't a case of *a posteriori* mapping of meanings to a pre-existing model but is already assumed to all objects and relations in the model. This is similar to the way that a person giving a sound argument by analogy presupposes the interpretations of the theoretical terms she refers to. At this point, it is reasonable to conclude, at least for our purposes in quantum theory, the following: a model is always interpreted – and its interpretation is included when the model, or theory for that matter, is built. This is concordant with all senses of 'model' we have discussed.

With this interim conclusion in place, it is time to wrap up what we can say about interpretations in the context of quantum mechanics. Next, I will present outlines for what an interpretation is and consider some counterarguments.

IV.III. A DEFINITION OF "INTERPRETATION"

Let us practice, once again, some rough categorization. All relevant senses of the notion of interpretation, with any kind of proper definition, can be primarily divided into two senses:

1. Semantical notions about the relation between a structure and its conditions, and
2. the ways theory and/or models are understood to refer to phenomena

As is instantly obvious from the preceding discussion, these are more often than not the same notion. Indeed, this categorization is put in place only to account for the use cases of interpretation where a semantical theory is not ascribed to by the author. Be that as it may, immediately pertaining to the discussion now examined, a set of statements is made in order to outline the notion of interpretation used in the domain of quantum theory. Many ques are taken from semantics and especially from model theory. The idea of extending interpretation from formal to non-formal sciences has been adopted from Carnap and Apostel.

However, no commitments to some syntactical system or some formal theory of models are made here. Moreover, the point of the following statements is not to fix the meaning of "interpretation" in some formal way or to some formal framework, but to suggest a definition based on how the term has been used in the relevant literature. The definition is composed of eight statements:

ID1. The notion 'interpretation' refers to, in all cases, to *what we take* any sign, expression, statement, model, theory, etc. to mean.
ID2. In the context of quantum theory, interpretation operates solely in reference to manipulation and observation of physical phenomena.
ID3. If quantum theory is applied in some specific physical situation, the theory is simultaneously interpreted.
ID4. The interpretation is presupposed in any application of the theory.
ID5. If quantum theory contains any model, the model is the particular application of formalism in the theory to a particular physical situation.
ID6. The act of model application is simultaneously the act of interpretation of theory.
ID7. The interpretation is *confirmed* if, when applied as a model to a situation the theory is interpreted to refer to, the formalism of the theory makes correct predictions about the components in the model.
ID8. Equivalently, the theory is *verified*, if it is interpreted into a model of a specific situation, and the model is confirmed by correct predictions about its components.[76]

Statements (ID1)-(ID4) are intentionally formulated in a way which does not necessitate the notion of 'model'. Conversely, statements (ID5)-(ID8) define the model as a product of interpretation. However, the view that a true interpretation equals a model, as is often the structure of formal model theory[77], is not adopted. This is because 'interpretation', at least for our intents and purposes, is an operational term, whereas 'model' is an abstract structure. From (ID5) and (ID6), however, we understand that if a model is at

[76] The notions *confirmation* and *verification* here require some explication. Confirmation in this context means that the interpretation of the theory is seen to be *right* in the most practical level – nothing above or around this sense is implied. For example, a physics student has the right interpretation of the Schrödinger equation if she is able to use it to correctly give a solution to a problem in a physics exam. 'Confirmation' is simply seeing that the theoretical content was correctly understood. Verification, equivalently, means that the theory is demonstrated to correctly produce the relevant predictions for the physical situation it is interpreted to refer to. 'Verification' has no implications for the theory to be 'true' in any stronger sense than what is defined here.

[77] See IV.II.II. The idea is that an interpretation *I* which makes all statements of theory *T* true is a model of *T*.

any time formed, it necessarily implies interpretation. I will only define this direction of implication, as it is not strictly necessary to state anything about interpretations implying models.[78]

Statement (ID1) serves to fix the *operational* nature of the notion of interpretation in general. Statement (ID2) gives the conditions for interpreting an abstract structure in quantum theory (i.e. restricts its domain to physically observable phenomena). Statement (ID3) fixes the act of interpretation (both logically and temporally) to theory application. The first half ends with statement (ID4), which states that interpretation precedes application. Now, (ID3) and (ID4) are very similar, but (ID4) is added to fix the said logical order.

In (ID7), we have the notion of interpretation confirmation. This can be stated so that if one understands the theory correctly, one applies the theory correctly. Thus, in the case of correct application, their interpretation of the theory is confirmed. In (ID8) we have the odder expression of the *verification of a theory*. By this, no commitments are made to verificationist projects of scientific justification. The term is used in the following trivial sense: if the theory checks out, i.e. it predicts phenomena correctly, we say by (ID7) and (ID8) that both the interpretation of the theory has been correct and the theory itself produces correct predictions.

This set of statements does not include one that says something about the theory being interpreted simultaneously with its construction. Although this is, of course, the case with quantum theory, such a concept is not necessary, having introduced the eight statements above, for a sufficient understanding of interpretations to come out.

Special attention is now paid to the expression “predictions about the components in the model”. This is included in order to further fix the reference of the model to physical phenomena. It is a proposed field-specific solution to the supposed problem of the intended model in quantum theory: the intended model is unavoidably designated by observations, because the *components* in the model are the ones that are compared against the measurements, not just models as simple entities. By components, we mean variables, constants, operators, functions etc., and *any mathematical relations formed by these*, partially isolated, as the complete form of the model, or in *any* other

[78] It can be argued that a given interpretation is evaluable only if it is represented in some way, and this representation can always be thought of as a model. This would mean that an interpretation always implies a model. However, for my argument concerning the nature of interpretation it is not required to say anything of this.

configuration. Consequently, a component can be a single term, or a system of multiple terms – which is to say, a system of components is also *a* component in the model. As result of this, observations can, in a very idealized (and fictional) example, confirm a single component in the model, i.e. from measurements it can be ascertained that the model predicts them, but the model cannot be, by the means of measurements, analyzed further to say something, for example, about the physical meaning or causal relations of its constituents. Thus, the model is confirmed, but only as this single component.

We'll call these components in this context simply *model components*. The model components themselves can be at this stage evaluated against their physical references, because a model is the result of interpretation. Model components are thus not simply technical expressions, but also statements about what the target system is like. These statements are then evaluated against empirical observations.

Of note is that the use of the notion 'component' here is completely isolated to serve this purpose only. It is separate from any other possible use cases in similar or different contexts. The motivation for the use of this notion is only to give more detail to the following general idea: the correctness of the model is evaluated against what it *says* about its target system. Thus, the *intention* behind any model, expression etc. plays a significant part on which model components are evaluated and in which way.

If observational means allow for isolating model components (as is, of course, the case up to the limit imposed by the uncertainty principle), whether single terms or partially isolated systems, then model confirmation for multiple components in the model is attained. In this case, the model is confirmed both for a single component, and also for some sub-components participating in the construction of the most general component. Of course, in the application of any model, we already know the predictions it makes in relation to measurements.[79] Consequently, we can study the model and its observation-corresponding components without any actual experimentation (we can thank one hundred years of quantum mechanics for this). Let us look at this more closely and take up the example of the quantum tunneling situation once more. As before, we get for the subtraction of potential and initial energy:

$$U - E < 0 \Rightarrow k^2 = -\frac{2m}{\hbar^2}(U - E) \quad and \quad U - E > 0 \Rightarrow q^2 = \frac{2m}{\hbar^2}(U - E)$$

[79] As a clarification: a proper prediction is such that allows one to state, before experimentation, what exactly has been predicted.

Here, the term on the left refers to the wave function outside the potential barrier and the term on the right to the wave function in it. Again, *U*[80] stands for potential, *E* for the energy of the wave packet, and *m* for the particle's mass.

Now, the classic example of quantum tunneling in practice is the use of scanning tunneling microscopes (STM's). Here, I will follow Julian Chen's *Introduction to Scanning Tunneling Microscopy* (2007). STM is used to create images of objects' surfaces at the atomic level (up to the definition of 0.1 nm). In STM, a conducting tip (made out of tungsten or platinum-iridium) is brought extremely close (to the distance of well under one nanometer) to the sample object's surface. The tip is connected to three perpendicular piezoelectric transducers[81], corresponding to *x, y* and *z*-axises. In our case, the z-axis is perpendicular to the sample object's surface. A sawtooth voltage is introduced to the *x*-piezo, and a ramp voltage to the *y*-piezo, causing the tip to 'scan' the *xy*-plane. At this distance, the wave functions of the electrons in the tip overlap with the wavefunctions of the objects' surface.

A *bias voltage* (meaning the threshold voltage for, in this case, the operation of tunneling to actualize) of around $\pm 0.01V - \pm 2V$ is introduced in reference to the sample object's surface. This allows for a tunneling current between the tip and the sample surface. If the bias voltage in the sample is $V>0$, the electrons from the tip tunnel to the sample (or, more precisely, to the empty states in the sample), and if $V<0$, the other way around. This current is then conducted from the tip to an amplifier. The current is then converted to a value of voltage, and the attained value is used to form a direct feedback loop with the piezoelectric transducer in the *z*-axis. In order to drive the *z*-piezo, the current is compared to a reference value. If, for example, the actual current is larger than the reference, the *z*-piezo is controlled, by application of voltage, to withdraw the tip further from the sample's surface, and vice versa. This process eventually establishes a proper definition.

From the current, then, we get *z*-values (converted by a certain formula to the metric system) for the *xy*-plane, which – for just the *x*-dimension – look like this:

[80] The letter *U* is used this time in order to not confuse the potential barrier with bias voltage.
[81] A *piezoelectric transducer* is an object which contracts or expands when a voltage is introduced. Symmetrically, introducing a mechanical force to the object creates a voltage in it. These transducers are used to control the tip: when a sawtooth voltage is applied in one axis and a ramp voltage in the other, it is hopefully clear to see which kind of motion the tip itself makes in the *xy*-plane.

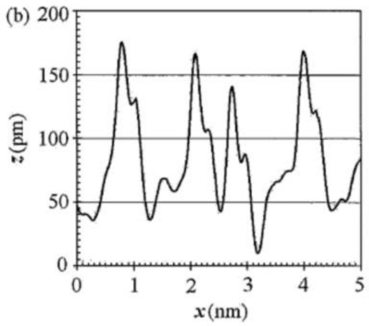

Figure 6: A plot of one-dimensional z-values in STM. (Chen 2007, 2.)

This plot is then interpreted as peaks representing atoms and valleys representing empty space. Now, with instrumentation out of the way, let's look at the quantum mechanics of the situation. The terms given above are, of course, solutions of the Schrödinger equation of form (to be clear, we are considering this case one-dimensionally in the z-axis):

$$\psi(z) = \psi(0)e^{\pm ikz}$$

Inside the potential barrier the solution is of the form q^2. This term describes the *decay* of the wave packet inside the barrier. As before, we get the probability density of observing the electron at location *z* from

$$\rho = \psi^*\psi = |\psi(0)|^2 e^{-2kz}.$$

Now, of course, the vacuum between the sample surface and the tip is an *insulator*, which creates a *potential barrier* between the two. If the distance between the two surfaces is too large, their respective wavefunctions decay into the vacuum and the tunnelling probability exponentially approaches zero. At a correct distance, however, some wave packets are transmitted through the barrier. We now define the *Fermi level,* which is the maximum number of occupied states in a metal, using the work function ϕ as the reference point:

$$E_F = -\phi,$$

where ϕ is the amount of energy required to release an electron from an object's surface into the vacuum. What this means is that in order to pass through the potential barrier classically, the energy state of an individual electron should be $E_N = E_F + \phi$, thus the "height" of the potential barrier is defined by the work function. However, when the bias voltage is introduced, we get for an individual wave packet:

$$\pm[(E_F - eV) \leq E_N \leq E_F],$$

and we can observe tunnelling from the occupied states in one surface to the empty states in the other. Keep in mind that the voltage bias introduced is much smaller than the work function. Now, we can introduce a transmission coefficient for the current that has been formed, which is the ratio between all wave packets "hitting" the potential barrier and those that penetrate it. Defining it from the sample surface to the scanning tip, we get:

$$T \equiv \frac{I(z)}{I(0)} = e^{-2qz}.$$

We get the initial current $I(0)$ directly from the bias voltage, and the current at $I(z)$ by reading the tip. Knowing that the potential barrier is defined by the work function, we now get for q:

$$q^2 = \frac{2m\phi}{\hbar}.$$

Knowing that this defines the decay of the wave packet inside the wave barrier, a numerical value for the decay constant may be derived. Further knowing that electrons can only tunnel to compatible empty states and applying geometry in reference to the *xy*-plane, the tunnelling current can be associated with the density of filled or empty states in the sample object's surface. A further exposé of the physics involved is not presented here, as this outlining suffices for our purposes.

At this stage the reader might be curious as to why they were just given a lecture on STM. This is all done to illustrate model components in this particular application of quantum theory. First up, we have the wave function, which gives the solution for wave packet decay in the barrier. This solution, of course, is derived from the Schrödinger equation for the wave function inside a potential U. The decay constant is then used to calculate the transmission ratio, which is a statistical term defined by current. Here, we are able to say something about the components involved in our models.

Our observations primarily confirm components such as the transmission ratio. The transmission ratio, on the right side of the equation, is defined by the wave function, which has its solution for q from the Schrödinger equation. Thus, we are able to confirm that the solution for q is correct, because it correctly predicts the expectation of transmission. Furthermore, we are able to say the following: 1) that this transmission occurs in the model because of wave packet overlap, and 2) from the rate of decay we know when the potential barrier is narrow enough for transmission to occur. The component *z* is, then, in conjunction with other components, confirmed to model whether any transmission is to be expected. This is also to say that the model component of ‘wave packet overlap’ is required to produce the correct predictions – in this model – whether any physical wave packets were present or not.

We are able to say something about the wave function as well. From it, we get the expectation values that are used in defining current $I(z)$. Because of its waveform, we are able to say, without commitment to else than prediction and manipulation of the system, that the wave function gives the probability amplitudes in the situation. (The probabilistic interpretation is already assumed in forming of the model.)

However, there is no possibility of interpreting the wave function as a physical entity, as no such component can be found from the utilized model. In other words, no such predictions are made by the model which had something to do with the ontological nature of the wave function.[82] A realistic counterargument to the preceding might be presented in the following way: the wave function is able to give the correct probability amplitudes only if it is a physical entity itself. However, the proponent of such an argument would need to somehow show that there exists a necessary connection between a given physical phenomenon and a model that correctly predicts it. This ontological commitment would also require one to explain away all the instances where a model of one system was used in predicting the behaviour of some other, completely different system (as discussed in IV.I.).

By this treatment, we understand the intended model as a successful application of theory, in which we are only considering the predictions in regard to measurements in a given physical situation. In the STM example, we interpret the base quantum theory in a very simple way – the quantum tunnelling case can be immediately seen from the Schrödinger equation – and map the formalism to phenomena successfully. However, from the equations presented above, it might not yet be very clear what kind of components in an otherwise predictive model are *not* confirmed – this is because the interpretation here is minimal. One of such components would be, if separately argued

[82] There is some contention to this, stemming from a somewhat related experiment. In the Aharonov-Bohm effect, an electrically charged particle is affected by electromagnetic potential despite the particle being in a region where both magnetic and electric fields are zero (Aharonov & Bohm 1959). The possibility of a physical electromagnetic potential has then been linked to quantum potential (Philippidis, Bohm & Kaye 1982). However, a quantum-mechanical solution to this phenomenon has also been presented (Pearle & Rizzi 2017a, Pearle & Rizzi 2017b).

for, the existence of a real wave between the surface and the tip. Importantly, while the component “wave function” is confirmed, the component “wave” is not – this is the boundary of our epistemic access. A situation where these components arise more clearly is discussed in the next section.

The ideas proposed here are not exactly novel. Asher Peres, in his 1978 paper *“Unperformed experiments have no results”*, writes, in reference to the Bell inequality and hidden variable theories:

> There are two possible attitudes in the face of these results. One is to say that it is illegitimate to speculate about unperformed experiments. […] For instance, it is not possible to formulate the EPR paradox. (Peres 1978, 746).

Regarding the problem of the intended model, a similar argument is in fact presented by Putnam in his 1980 paper *“Models and Reality”*. The paper discusses intended models in model theory but arrives at similar conclusions with this thesis. The problem Putnam addresses is the Löwenheim-Skolem paradox, which is roughly the following: every countable non-contradictory set theory has a countable model, but, from the same axioms, it is possible to derive a statement that says that there exists an uncountable set. Consequently, or so it seems, it is possible that a countable model satisfies the sentence “there are uncountable sets”. Because a model of a theory is such an interpretation which makes all the statements of the theory true, a countable model is contradictory with this statement.

The main point here is that, strictly in the formal system, different (and contradictory) models can be derived (i.e. interpreted) from the same theory. Without delving further into the mathematics, the crux of the issue is, according Putnam, what we take to be the meaning of a sentence. The problem arises when set-theoretical terms are treated as having independent existence (i.e. in realist terms) – but when they are tied to use and what we know of their reference and the world no paradoxes appear. Putnam writes (1980, 481):

> The problem, however, lies with the predicament itself. The predicament only is a predicament because we did two things: first, we gave an account of under- standing the language in terms of programs and procedures for *using* the language (what else?); then, secondly, we asked what the possible "models" for the language were, thinking of the models as existing "out there" *independent of any description*. At this point, something really weird had already happened, had we stopped to notice. On any view, the understanding of the language must determine the reference of the terms, or, rather, must determine the reference given the

> context of use. If the use, even in a fixed context, does not determine reference, then use is not understanding. The language, on the perspective we talked ourselves into, has a full program of use; but it still lacks an *interpretation*.

The main point is that there are no “realist” truth-conditions for any statement. From formal set theory, then, we get very close to our understanding of interpretation of quantum theory (Putnam 1980, 481— 482):

> To adopt a theory of meaning according to which a language whose whole use is specified still lacks something - viz. its "interpretation"- is to accept a problem which can only have crazy solutions. To speak as if *this* were my problem, "I know how to use my language, but, now, how shall I single out an interpretation?" is to speak nonsense. Either the use *already* fixes the "interpretation" or *nothing* can.

Some very important remarks need to be made here. Firstly, Putnam’s argumentation, if applied directly to our case, seems to say that the interpretation of theory is already fixed once the theory is formed. Although this is arguably the case with quantum theory as well, this line of argumentation is not strictly required here. Secondly, the idea of confirming components in a model against observations might seem to run against Putnam’s argumentation, as he further writes (Putnam 1980, 482):

> Nor do "causal theories of reference", etc., help. Basically, trying to get out of this predicament by *these* means is hoping that the *world* will pick one definite extension for each of our terms even if *we* cannot. But the world does not pick models or interpret languages. *We* interpret our languages or nothing does.

However, there is no contradiction here. The models, if the notion of models is needed, are interpretations of a theory – consequently model confirmations are confirmations about the interpretation of a theory being correct. Obviously, the theory itself, in the form it is in, is made by people. The theory is, however, only possible to construct in reference to physical phenomena. This is to say: while *we* assign the reference of our theories, the reference can only be assigned to what can be perceived of the world. To be clear: here we are very far from any kind of causal theory of reference.

Next, the “interpretations of quantum theory” are examined in light of the conclusions made here.

IV.IV. INTERPRETATIONS OF QUANTUM THEORY

Now, finally, the attention turns back to the problem presented in III.III., namely, the dispute between (OEE) and (CO&H).[83] Let us look at the two interpretations presented in this thesis – namely the Copenhagen interpretation and the de Broglie-Bohm interpretation – in light of what is now outlined as the notion of interpretation of quantum theory.

It needs to be stressed that 'Copenhagen' is an umbrella term which is not well defined. As was made clear in chapter II, the version addressed here is based on the work done by Heisenberg and Bohr in the 1920's and 30's. It also needs to be noted that the notion of "interpretation" is, from the get-go, problematic, as it was not used by Heisenberg or Bohr – Heisenberg referred to the "Copenhagen *geist* of quantum theory" (Heisenberg 1930, preface). And, as seen in chapter II, there is really nothing on top of the standard quantum theory here. The notion "Copenhagen attitude" will be consequently used here. The Copenhagen attitude includes, at least, these statements regarding the nature of quantum theory:

CS1. The uncertainty relations point to a fundamental uncertainty[84] which cannot be circumvented.
CS2. No unobserved quantities of a theory have physical meaning.
CS3. If a calculation is used to generate the physical history of the electron, it is not a matter of science but of personal belief.
CS4. The wave and particle pictures are not to be taken as physical realities of a quantum-physical system.
CS5. The phenomenon, as far as the uncertainty principle is concerned, is irrevocably the result of the whole experiment, and as such, unanalysable into further causal parts.

Many, if not all, stances here are the consequence of adopting the uncertainty principle as fundamental. From the Kochen-Specker theorem we know, as far as quantum theory is concerned, that the uncertainty principle is indeed inescapable. Now, in the framework of interpretation presented above, these statements – each and every one of them – also become direct consequences of mapping the theory to the phenomena the theory is taken to represent. Thus, we have an almost trivial vindication of the Copenhagen statements, as presented here, as they are in-built in the way we understand interpreting.

Although this is surely clear enough, let us look, as an example, statement (CS2). This is connected with statements (CS7) and (CS8). Upon interpreting the theory into a

[83] See page 49.
[84] Here, of course, one needs to define this as *fundamental to the theory.*

certain physical situation, we assign an interpretation to only those components in the model which we can map to measurements. Of course, the *manner in which* the components are mapped to phenomena are various. In the STM case, for example, we saw that the wave function is confirmed to be a correct instrument for predictions, whereas we are able to read the current $I(z)$ from the tip. The bottom line is that the measurement determines two things:

1. Can anything be said about a given component; and
2. what can be said about a given component,

and the act of model confirmation is then executed, at all times, in reference to these conditions.

This might seem like a very short and trivial examination of the Copenhagen attitude. This is because there is really nothing in the statements above that are not part of quantum theory, when stripped to its minimum content by eliminating all extra-empirical reference. These themes have already been studied thoroughly. And in order to avoid confusion and redundancy, other views sometimes considered as part of "the Copenhagen interpretation" (e.g. Peres, 1999) are not included here. In any case, it is hopefully clear enough that it is highly problematic to think of this as a separate "Copenhagen" interpretation of quantum theory.

What remains to be done now is to measure the score with the de Broglie-Bohm interpretation. As seen in chapter III, this framework introduces novel mathematical formulations to the standard procedure of quantum theory. Mainly, we have the polar wave function as solution to the "quantum Hamilton-Jacobi", with the term Q isolated as the surplus term that is added to the classical potential. Physically speaking, we are given several different meanings to different terms:

- $Q = \frac{\hbar^2 \nabla^2 R}{2mR}$ ≡ quantum potential
- $\psi = R\mathrm{e}^{\left(\frac{iS}{\hbar}\right)}$ ≡ quantum field
- $\vec{v}(\vec{r}, t) = \frac{\nabla S(\vec{r},t)}{m}$ ≡ guidance equation

These are isolated terms in the overall structure which is non-contradictory with standard quantum mechanics. However, we instantly see that these are proposed *model components.* If they are derived from quantum theory by means of correct application of the theory, they should correspond to our measurements *in the way* we interpreted the theory to correspond to them. Here, of course, we run into problems.

As an easy example, let us stretch our imagination a little and think of a situation where an application of quantum theory involved a mathematical model with the term Q taken as a measured quantity. In this situation a scientist would like to examine the form of the quantum potential of electrons tunnelling through the potential barrier like in the STM example presented above. Then, by introducing a special device, called the "quantum potential examiner", located near the tip, the quantum fields ψ associated with each electron either reflected from or transmitting through the barrier are read, and from these quantum fields, the scientist could construct the quantum potential, which should correspond to predicted values. At this point, the scientist could say that the model component Q is confirmed to be properly applied from quantum theory, and that she has interpreted quantum theory correctly.

Because of the nature of the quantum potential in the Hamilton-Jacobi formulation, however, an experiment such as this cannot be performed. Interpreting Q into an experiment such as this would be akin to attempting to measure the wave function, which would be an incorrect interpretation of quantum theory. This statement might give some pause: both Q and ψ are already known when the experimental arrangement is known. Thus, one might argue, that a physical experiment for Q can be indeed conducted (in the same indirect sense as for ψ). However, physically measurable quantities cannot distinguish the terms of Q from any other sequence equivalently non-contradictory with quantum theory. Another way of saying this is that physical experimentation cannot *isolate* Q in any such way that vindicated the descriptive statements associated with it.[85]

If, on the other hand, we agree that there is no experiment for Q, nor does Q have any role in predicting phenomena, then it is also agreed Q is not a result of interpreting quantum theory (in the sense 'interpretation' is defined here). This requires some illumination. It is not the intent here to claim that the quantum potential or the phase function (among other properties of the dB-B) did not do any work in predicting quantum phenomena correctly. Such a claim would evidently be false, because the omission of these properties in the de Broglie-Bohm formalism would cause its predictions to be false or nonsensical. Of course, these remarks apply to ψ as well.

[85] The situation is different for the probabilistic interpretation of the wave function. Its physical nature is never committed to – it is only required to ascertain that the values of it are correct by producing correct probability distributions.

However, recall that a model can be evaluated in as many components as can be intelligibly specified. In the case of the derivation presented by Bohm in 1952, what is confirmed is that the guidance equation and the Hamilton-Jacobi, attained by the polar form of the wavefunction, correctly predict the observations of atomic physics. Thus, quantum theory can indeed be correctly interpreted in this way. However, further analysing these equations into ∇S, Q, etc., attention is required. To confirm the positive ontological status of these individual components, a demonstration would be required that phenomena *associated with them* could not be *predicted without them*. If a formulation, derived from the same theory, demonstrates the opposite, then such model components cannot be confirmed. Thus, they do not strictly follow from the *interpretation* of the theory.

The preceding four paragraphs might be the most controversial ones in this thesis, because the idea that the wave function cannot be observed has been challenged by the development of *weak measurements*. Weak measurement, roughly characterized, is a general term for measurements of a quantum system designed to disturb the system as little as possible. The varieties of proposals that present a challenge to the preceding arguments can be divided into two: the so-called *protective* measurement techniques introduced by Aharonov and Vaidman in 1993 and the modified Stern-Gerlach measurement sequence discussed, for example, in Flack & Hiley (2014), Flack & Hiley (2015) and Hiley & Van Reeth (2018). Both approaches are an important topic of modern quantum mechanics with regrettably little room in this thesis. However, neither approach (while being very distinct from each other) demonstrates a circumvention of uncertainty relations – which would be required in measuring Q *or* ψ in our STM example. More importantly, there is reason to argue that such a demonstration would indeed *refute* quantum theory – thus the former would not be an interpretation of the latter.[86]

Moving back to more formal treatment of interpretation, a kind of model-theoretic argument for the de Broglie-Bohm interpretation might still be given. This exact argument has not, for the best of my knowledge, been presented in relevant literature, but it would be roughly of the following form:

[86] This having been said, the notion of "direct wave packet measurement" in the wake of Aharonov & Vaidman (1993) and the correlations of macroscopic tracks with simulated Bohm trajectories in Hiley & van Reeth (2018) do deserve a lot more attention and dissection that can be given here.

M1. Quantum theory, purely in and of itself, is an undefined sequence of signs, sentences, etc.
M2. An interpretation fixes the reference of the theory, and is a model, or a set of models, of the theory, if the interpretation verifies the theory in reference to phenomena.
M3. The de Broglie-Bohm interpretation, or, the set of models it contains, successfully connects quantum theory to empirical phenomena in its own mathematical framework.
M4. Consequently, the de Broglie-Bohm interpretation is both a model and an interpretation of quantum theory.

The straight-forward rebuttal of this argument has to do with the *intention*[87] of the de Broglie-Bohm interpretation in connection with understanding model components. Now, it needs to be strongly emphasized that there is no technical procedure (such as a causal theory of reference, etc.) for determining when a model is confirmed in connection to its components and when it is not, but this does not pose a problem, because the intention is provided by the author of the model.[88] In this instance, the intention behind the key model components in the de Broglie-Bohm interpretation is that they are given an explicit physical meaning. Thus, we can fix the standard of model confirmation in reference to these model components.

Applying our rules for interpretation of quantum theory – with knowledge of the epistemic limitations of quantum theory – it follows that assigning these kinds of components does not have epistemic backing. Consequently, a counter-argument to the preceding is of the following form:

cM1. All signs, sentences etc. of quantum theory are interpreted when they are applied to represent physical phenomena.
cM2. A successful application confirms our interpretation of theory to be correct.
cM3. The model we use to connect the theory to physical phenomena is confirmed as the components it contains.
cM4. The ontological model components in the de Broglie-Bohm interpretation can be omitted without effect to quantum theory (in another way of applying quantum theory).
cM5. Thus, these components are not confirmed as correct interpretations of quantum theory.

It is evident that ‘interpretation’, as I suggest it to be understood, is very strict. This way, when interpreting quantum theory, very little room is left for matters of taste. Consequently, if a proponent of the de Broglie-Bohm interpretation held on to a strict

[87] This is not to be confused with the idea of *intended models*.
[88] In order to avoid confusion: the model is always measured against phenomena. But there are many uses for a model – for instance, atomic motion can be modelled with spring systems, but this model is only valid when it is meant to be an instrument instead of a real physical description. Conversely, if the model is intended to state the existence of tiny springs, the model is invalid. What is *intended* fixes what is *reviewed.*

understanding of interpretation (as suggested in this thesis), positive ontological statements should be ruled out of its interpretative content. However, letting go of these statements completely would likely be unacceptable for a proponent of the de Broglie-Bohm interpretation. How should these components, then, be understood? I will briefly suggest an alternative way in conclusions.

V. CONCLUSIONS

A summarization of the train of thought in this thesis can now be made.

CC1. All that can be said about the world in the context of physics is the predictions our physical theories make, and quantum physics is no exception.

CC2. The Copenhagen interpretation is not a distinct interpretation of quantum theory, but just a general spirit of limiting our statements to the ones that can be justifiably derived from the theory.

CC3. It is proven by the von Neumann and Kochen-Specker theorems that violating the uncertainty principle inside quantum theory is impossible.

CC4. There is no other theory pertaining to sub-atomic phenomena than quantum theory.

CC5. An interpretation of quantum theory is the correct understanding and application of it in reference to physical interactions.

The first conclusion is a reaffirmation of physics as a positivistic science, while (CC2) recognizes the near-trivial interpretative content of the Copenhagen “spirit”. (CC3) and (CC4) together ground the fact that if we had a way of violating the uncertainty principle, that would be a part of some other theory than quantum theory. Finally, (CC5) is shortly what is suggested to be the meaning of the notion “interpretation”.

The proposed standards of interpretation are not necessarily the definitive truth, but they form a robust philosophical basis for the concept in quantum mechanics. If interpretations are understood in this way, then it follows that the ontological content of the de Broglie-Bohm interpretation is *not the result of interpreting* quantum theory. If one were to disagree with the preceding, then one would need to show exactly how to understand the notion of interpretation so that the aforementioned ontological statements are vindicated.

In the wake of the modern quantum theory, especially onwards from the Solvay conference of 1927, two opposite attitudes towards the research of sub-atomic phenomena started to form – those of *restriction* and *aspiration*. Einstein abandoned his aspirations for a more complete description of sub-atomic processes as his thought experiments failed. After these events, the concept of “interpretations” was introduced to designate alternative formulations of quantum theory that had differing views with the base theory's implications but not its predictions, such as the core[89] of the de Broglie-Bohm interpretation. Despite this, it is important to note that the connection to novel experiments has always been present in the central authors of the interpretation. Bohm brought up the possibility of novel causal mechanisms the sub-quantum level already in Bohm (1952b, 184—185). The work of Hiley et. al. in the 2010s, especially, continues the search for a novel experiment. Thus, importantly, the difference between Einstein and several authors on the de Broglie-Bohm interpretation is not exactly clear-cut.

However, historically this conceptual separation of attitudes can be understood by the fact that before early quantum physics the connection between epistemology and ontology was more straightforward: there were no theoretical obstacles to be found in improving accuracy of measurement (limitations were a matter of practice, not principle). After the formation of quantum theory the tension was then formed, roughly, between the following two preferences:

1. Limiting physics as a strictly epistemological enterprise and pertaining to what could be observed by the means of it, and
2. striving towards a more complete description of reality as such.

The proponent of the preference (1) can argue that the preference (2) dangers a shift to metaphysics; that is, *what is beyond physics*. And naturally, the proponent of preference (2) is right to argue that preference (1) has the problem of defining what ultimately can be observed – possibly leading to contradictions with the underlying empirical attitude. Moreover, it is important to acknowledge that the terms "philosophical" and "interpretation" are not completely neutral terms. Due to the social development in the physics community in the 20th century, naming something *a priori* an "interpretation" can include the assumption that the competing view does not have any novel epistemic value (Pinch 1977, e.g. 177). The notion of "philosophizing" can also be used to push the author of some formulation outside of physics (Hanson in Bohm et. al. 1962, 89—90.) These hostile social dimensions to the use of the term "interpretation" seem to highlight the importance of limiting its actual range, as is done in this thesis.

As my final statement, I argue that through the discussions in this thesis we see a potentially fruitful alternative way of understanding the ontological content in the de Broglie-Bohm interpretation. A realistic description is preferred by many of its

[89] Using the polar form of the wave function to attain the quantum Hamilton-Jacobi.

proponents, of course, because it provides an *explanation* of the quantum phenomena. Along these lines were Bohm and Hiley themselves:

> In this way we explain why the opening of a second slit can prevent particles from arriving at points […] (Bohm & Hiley 1993, 32.)
>
> In this explanation of the quantum properties of the electron, the fact that the quantum potential depends only on the form and not on the amplitude of the quantum field is evidently of crucial significance. (Bohm & Hiley 1993, 35.)

Especially Bricmont doubles down on this. His whole idea of what he calls the de Broglie-Bohm theory is that it provides a scientific explanation of quantum mechanics (Bricmont 2016, 161—162):

> [I]t is the de Broglie-Bohm theory that *explains* why ordinary quantum mechanics is sufficient [for all practical purposes], something that is true but mysterious without de Broglie-Bohm.

It could be, then, reasonable to think of the ontological content in the de Broglie-Bohm as an *explanation* rather than an *interpretation*. A natural follow-up to this would be then to examine the nature of explanation at hand. Regarding scientific explanations in the context of natural sciences, the structure of the de Broglie-Bohm interpretation could be studied as a *causal* explanation.

One of the goals of the de Broglie-Bohm interpretation, as is at this point well understood, is to give a description of what happens at a very primitive level of reality relating to phenomena described by quantum theory. It follows that this description is *realistic*, at least when thought of as a hypothesis, as Bohm did. Moreover, it includes numerous novel causal claims. For instance, active information is fed by the pilot wave to the particle, which modulates its behaviour with respect to the content of this information. This is possible because the particle has an inner structure capable of processing information. In any experimental setup, this active information is included in the quantum potential.

This seems to have potential to be a more fruitful way to understand interpretations of quantum mechanics in general. As long as quantum theory is the only theory of sub-atomic phenomena, it follows that interpretations of it share its constraints. Thus, accounts that go “above and beyond”, as it were, must do something on top of interpreting. Whether that something is new predictions or novel ontological implications, they differ from interpreting quantum theory – the former, very likely, contradicting it. It may well be the case that not all or even most interpretations of quantum mechanics seek to explain – but those containing ontological accounts might. One then ends up with natural follow-up questions. How does the de Broglie-Bohm interpretation look as a causal explanation? Are there other competing explanations among interpretations of quantum mechanics – for instance, does the many-worlds interpretation attempt a comparable ontology?

There is more work to be done for better philosophical understanding of interpretations. Moreover, the crucial study of the epistemological basis of quantum theory is far from over – even if it has been slightly forgotten after the early days of the branch.

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